(*Diadema*) bore their long barbed stings into the flesh of the foot, where they break off and remain, inflicting painful and dangerous wounds But the worst of all injuries to the skin are inflicted by the coral rocks themselves. The myriads of hard points and edges with which they are armed inflict numberless wounds on the hands which attempt to uproot them."

"I never in my life had such a lacerated and smarting skin as after a few days diving and coral fishing at the Point de Galle. The wounds did not heal for several weeks. But what were such temporary sufferings as these in comparison with the wealth of new impressions and delights with which this visit to the wonderful coral-banks of Ceylon enriched my whole future life!"

THE BRITISH ASSOCIATION

THE fifty-second annual meeting of the British Association was opened yesterday at Southampton, when Sir John Lubbock resigned the presidency to Dr. C. W. Siemens, F.R.S., the president-elect. We have already given such full details concerning the arrangements, that at this stage little more remains to be said. All the provisions made by the local committee appear to be quite satisfactory, and although we cannot expect the attendance to be so large as at the Jubilee last year, still some eminent foreign men of science are expected—Helmholtz, Clausius, Du Bois Reymond, J. P. Cook, Langley, Von Rath, Baumhauer, and others.

INAUGURAL ADDRESS BY C. WILLIAM SIEMENS, D.C.L. (OXON), LL.D. (GLASG. AND DUBL.), PH.D., F.R.S., F.C.S., MEMBER INST. C.E., PRESIDENT

IN venturing to address the British Association from this chair, I feel that I have taken upon myself a task involving very serious responsibility. The Association has for half a century fulfilled the important mission of drawing together, once every year, scientists from all parts of the country for the purpose of discussing questions of mutual interest, and of cultivating those personal relations which aid so powerfully in harmonising views, and in stimulating concerted action for the advancement of science.

A sad event casts a shadow over our gathering. While still mourning the irreparable loss Science had sustained in the person of Charles Darwin, whose bold conceptions, patient labour, and genial mind made him almost a type of unsurpassed excellence, telegraphic news reached Cambridge just a month ago, to the effect that our Honorary Secretary, Professor F. M. Balfour, had lost his life during an attempted ascent of the Aiguille Blanche de Penteret. Although only thirty years of age, few men have won distinction so rapidly and so deservedly. After attending the lectures of Michael Foster, he completed his studies of Biology under Dr, Anton Dohrn at the Zoological Station of Naples in 1875. In 1878 he was elected a Fellow, and in November last a member of the Council of the Royal Society, when he was also awarded one of the Royal Medals for his embryological researches. Within a short interval of time Glasgow University conferred on him their honorary degree of LL.D., he was elected President of the Cambridge Philosophical Society, and after having declined very tempting offers from the Universities of Oxford and Edinburgh he accepted a professorship of Animal Morphology created for him by his own University. Few men could have borne without hurt such a stream of honourable distinctions, but in young Balfour genius and independence of thought were happily blended with industry and personal modesty; these won for him the friendship, esteem, and admiration of all who knew him.

Since the days of the first meeting of the Association in York in 1831, great changes have taken place in the means at our disposal for exchanging views, either personally or through the medium of type. The creation of the railway system has enabled congenial minds to attend frequent meetings of those special Societies, which have sprung into existence since the foundation of the British Association, amongst which I need only name here the Physical, Geographical, Meteorological, Anthropological, and Linnean, cultivating abstract science, and the Institution of Mechanical Engineers, the Institution of Naval Architects, the Iron and Steel Institute, the Society of Telegraph Engineers and Electricians, the Gas Institute, the Sanitary Institute, and the Society of Chemical Industry, representing applied science. These meet at frequent intervals in London, whilst others, having similar objects in view, hold their meetings at the University towns, and at other centres of intelligence and industry throughout the country, giving evidence of great mental activity, and producing some of those very results which the founders of the British Association wished to see realised. If we consider further the extraordinary development of scientific journalism which has taken place, it cannot surprise us when we meet with expressions of opinion to the effect that the British Association has fulfilled its mission, and should now yield its place to those special Societies it has served to call into existence. On the other hand, it may be urged that the brilliant success of last year's Anniversary Meeting, enhanced by the comprehensive address delivered on that occasion by my distinguished predecessor in office, Sir John Lubbock, has proved, at least, that the British Association is not dead in the affection of its members, and it behoves us at this, the first ordinary gathering in the second half century, to consider what are the strong points to rely upon for the continuance of a career of success and usefulness.

If the facilities brought home to our doors of acquiring scientific information have increased, the necessities for scientific inquiry have increased in a greater ratio. The time was when science was cultivated only by the few, who looked upon its application to the arts and manufactures as almost beneath their consideration; this they were content to leave in the hands of others, who, with only commercial aims in view, did not aspire to further the objects of science for its own sake, but thought only of benefiting by its teachings. Progress could not be rapid under this condition of things, because the man of pure science rarely pursued his inquiry beyond the mere enunciation of a physical or chemical principle, whilst the simpler practitioner was at a loss how to harmonise the new knowledge with the stock of information which formed his mental capital in trade.

The advancement of the last fifty years has, I venture to submit, rendered theory and practice so interdependent, that an intimate union between them is a matter of absolute necessity for our future progress. Take, for instance, the art of dyeing, and we find that the discovery of new colouring matters derived from waste products, such as coal-tar, completely changes its practice, and renders an intimate knowledge of the science of chemistry a matter of absolute necessity to the practitioner. In telegraphy and in the new arts of applying electricity to lighting, to the transmission of power, and to metallurgical operations, problems arise at every turn, requiring for their solution not only an intimate acquaintance with, but a positive advance upon electrical science, as established by purely theoretical research in the laboratory. In general engineering the mere practical art of constructing a machine so designed and proportioned as to produce mechanically the desired effect, would suffice no longer. Our increased knowledge of the nature of the mutual relations between the different forms of energy makes us see clearly what are the theoretical limits of effect; these, although beyond our absolute reach, may be looked upon as the asymptotes to be approached indefinitely by the hyperbolic course of practical progress, of which we should never lose sight. Cases arise, moreover, where the introduction of new materials of construction, or the call for new effects, renders former rules wholly insufficient. In all these cases practical knowledge has to go hand in hand with advanced science in order to accomplish the desired end.

Far be it from me to think lightly of the ardent students of nature who, in their devotion to research, do not allow their minds to travel into the regions of utilitarianism and of self-interest. These, the high priests of science, command our utmost admiration; but it is not to them that we can look for our current progress in practical science, much less can we look for it to the "rule of thumb" practitioner, who is guided by what comes nearer to instinct than to reason. It is to the man of science, who also gives attention to practical questions, and to the practitioner, who devotes part of his time to the prosecution of strictly scientific investigations, that we owe the rapid progress of the present day, both merging more and more into one class, that of pioneers in the domain of nature. It is such men that Archimedes must have desired when he refused to teach his disciples the art of constructing his powerful ballistic engines, exhorting them to give their attention to the principles involved in their construction, and that Telford, the founder of the Institution of Civil Engineers, must have had in his mind's eye, when he defined civil engineering as "the art of directing the great sources of power in nature."

These considerations may serve to show that although we see the men of both abstract and applied science group themselves in minor bodies for the better prosecution of special objects, the points of contact between the different branches of knowledge are ever multiplying, all tending to form part of a mighty tree—the tree of modern science—under whose ample shadow its cultivators will find it both profitable and pleasant to meet, at least once a year; and considering that this tree is not the growth of one country only, but spreads both its roots and branches far and wide, it appears desirable that at these yearly gatherings other nations should be more fully represented than has hitherto been the case. The subjects discussed at our meetings are without exception of general interest, but many of them bear an international character, such as the systematic collection of magnetic, astronomical, meteorological, and geodetical observations, the formation of a universal code for signalling at sea, and for distinguishing lighthouses, and especially the settlement of scientific nomenclatures and units of measurement, regarding all of which an international accord is a matter of the utmost practical importance.

As regards the measures of length and weight it is to be regretted that this country still stands aloof from the movement initiated in France towards the close of last century; but, considering that in scientific work metrical measure is now almost universally adopted, and that its use has been already legalised in this country, I venture to hope that its universal adoption for commercial purposes will soon follow as a matter of course. The practical advantages of such a measure to the trade of this country would, I am convinced, be very great, for English goods, such as machinery or metal rolled to current sections, are now almost excluded from the continental market, owing to the unit measure employed in their production. The principal impediment to the adoption of the metre consists in the strange anomaly that although it is legal to use that measure in commerce, and although a copy of the standard metre is kept in the Standards' Department of the Board of Trade, it is impossible to procure legalised rods representing it, and to use a non-legalised copy of a standard in commerce is deemed fraudulent. Would it not be desirable that the British Association should endeavour to bring about the use in this country of the metre and kilogramme, and, as a preliminary step, petition the Government to be represented on the International Metrical Commission, whose admirable establishment at Sèvres possesses, independently of its practical work, considerable scientific interest, as a well-found laboratory for developing methods of precise measurement.

Next in importance to accurate measures of length, weight, and time, stand, for the purposes of modern science, those of electricity.

The remarkably clear lines separating conductors from non-conductors of electricity, and magnetic from non-magnetic substances, enable us to measure electrical quantities and effects with almost mathematical precision; and, although the ultimate nature of this, the youngest scientifically investigated form of energy, is yet wrapt in mystery, its laws are the most clearly established, and its measuring instruments (galvanometers, electrometers, and magnetometers), are amongst the most accurate in physical science. Nor could any branch of science or industry be named in which electrical phenomena do not occur, to exercise their direct and important influence.

If, then, electricity stands foremost amongst the exact sciences, it follows that its unit measures should be determined with the utmost accuracy. Yet, twenty years ago very little advance had been made towards the adoption of a rational system. Ohm had, it is true, given us the fixed relations existing between electromotive force, resistance, and quantity of current; Joule had established the dynamical equivalent of heat and electricity, and Gauss and Weber had proposed their elaborate system of absolute magnetic measurement. But these invaluable researches appeared only as isolated efforts, when, in 1862, the Electric Unit Committee was appointed by the British Association, at the instance of Sir William Thomson, and it is to the long-continued activity of this Committee that the world is indebted for a consistent and practical system of measurement, which, after being modified in details, received universal sanction last year by the International Electrical Congress assembled at Paris.

At this Congress, which was attended officially by the leading physicists of all civilised countries, the attempt was successfully made to bring about a union between the statical system of measurement that had been followed in Germany and some other countries, and the magnetic or dynamical system developed by the British Association, also between the geometrical measure of resistance, the (Werner) Siemens unit, that had been generally adopted abroad, and the British Association unit intended as a multiple of Weber's absolute unit, though not entirely fulfilling that condition. The Congress, while adopting the absolute system of the British Association, referred the final determination of the unit measure of resistance to an International Committee, to be appointed by the representatives of the several Governments; they decided to retain the mercury standard for reproduction and comparison, by which means the advantages of both systems are happily combined, and much valuable labour is utilised; only, instead of expressing electrical quantities directly in absolute measure, the Congress has embodied a consistent system, based on the Ohm, in which the units are of a value convenient for practical measurements. In this, which we must hereafter know as the "practical system," as distinguished from the "absolute system," the units are named after leading physicists, the Ohm, Ampère, Volt, Coulomb, and Farad.

I would venture to suggest that two further units might, with advantage, be added to the system decided on by the International Congress at Paris. The first of these is the unit of magnetic quantity or pole. It is of much importance, and few will regard otherwise than with satisfaction the suggestion of Clausius that the unit should be called a "Weber," thus retaining a name most closely connected with electrical measurements, and only omitted by the Congress in order to avoid the risk of confusion in the magnitude of the unit current with which his name had been formerly associated.

The other unit I should suggest adding to the list is that of power. The power conveyed by a current of an Ampère through the difference of potential of a Volt is the unit consistent with the practical system. It might be appropriately called a Watt, in honour of that master mind in mechanical science, James Watt. He it was who first had a clear physical conception of power, and gave a rational method of measuring it. A Watt, then, expresses the rate of an Ampère multiplied by a Volt, whilst a horse-power is 746 Watts, and a Cheval de Vapeur 735.

The system of electro-magnetic units would then be :—

(1) Weber, the unit of magnetic quantity $= 10^8$ C.G.S. Units.
(2) Ohm ,, ,, resistance $= 10^9$,,
(3) Volt ,, ,, electromotive force $= 10^8$,,
(4) Ampère ,, ,, current $= 10^{-1}$,,
(5) Coulomb ,, ,, quantity $= 10^{-1}$,,
(6) Watt ,, ,, power $= 10^7$,,
(7) Farad ,, ,, capacity $= 10^{-9}$,,

Before the list can be looked upon as complete two other units may have to be added, the one expressing that of magnetic field, and the other of heat in terms of the electro-magnetic system. Sir William Thomson suggested the former at the Paris Congress, and pointed out that it would be proper to attach to it the name of Gauss, who first theoretically and practically reduced observations of terrestrial magnetism to absolute measure. A Gauss will, then, be defined as the intensity of field produced by a Weber at a distance of one centimetre; and the Weber will be the absolute C.G.S. unit strength of magnetic pole. Thus the mutual force between two ideal point poles, each of one Weber strength held at unit distance asunder, will be one dyne; that is to say, the force which, acting for a second of time on a gramme of matter, generates a velocity of one centimetre per second.

The unit of heat has hitherto been taken variously as the heat required to raise a pound of water at the freezing-point through 1° Fahrenheit or Centigrade, or, again, the heat necessary to raise a kilogramme of water 1° Centigrade. The inconvenience of a unit so entirely arbitrary is sufficiently apparent to justify the introduction of one based on the electro-magnetic system, viz., the heat generated in one second by the current of an Ampère flowing through the resistance of an Ohm. In absolute measure its value is 10^7 C.G.S. units, and, assuming Joule's equivalent as 42,000,000, it is the heat necessary to raise 0·238 grammes of water 1° Centigrade, or, approximately, the $\frac{1}{1000}$th part of the arbitrary unit of a pound of water raised 1° Fahrenheit and the $\frac{1}{4000}$th of the kilogramme of water raised 1° Centigrade. Such a heat unit, if found acceptable, might with great propriety, I think, be called the Joule, after the man who has done so much to develop the dynamical theory of heat.

Professor Clausius urges the advantages of the statical system of measurement for simplicity, and shows that the numerical values of the two systems can readily be compared by the introduction of a factor, which he proposes to call the critical velocity; this, Weber has already shown to be nearly the same as the velocity of light. It is not immediately evident how by the introduction of a simple multiple, signifying a velocity, the statical can be changed into dynamical values, and I am indebted to my friend Sir William Thomson for an illustration which struck me as remarkably happy and convincing. Imagine a ball of conducting matter so constituted that it can at pleasure be caused to shrink. Now let it first be electrified and left insulated with any quantity of electricity on it. After that, let it be connected with the earth by an excessively fine wire or a not perfectly dry silk fibre; and let it shrink just so rapidly as to keep its potential constant, till the whole charge is carried off. The velocity with which its surface approaches its centre is the electrostatic measure of the conducting power of the fibre. Thus we see how "conducting power" is, in electrostatic theory, properly measured in terms of a velocity. Weber had shown how, in electromagnetic theory, the resistance, or the reciprocal of the conducting power of a conductor, is properly measured by a velocity. The critical velocity, which measures the conducting power in electrostatic reckoning and the resistance in electromagnetic, of one and the same conductor, measures the number of electrostatic units in the electromagnetic unit of electric quantity.

Without waiting for the assembling of the International Committee charged with the final determination of the Ohm, one of its most distinguished members, Lord Rayleigh, has, with his collaborateuse, Mrs. Sidgwick, continued his important investigation in this direction at the Cavendish Laboratory, and has lately placed before the Royal Society a result which will probably not be surpassed in accuracy. His redetermination brings him into close accord with Dr. Werner Siemens, their two values of the mercury unit being 0·95418 and 0·9536 of the B.A. unit respectively, or 1 mercury unit $= 0{\cdot}9413 \times 10^9$ C.G.S. units.

Shortly after the publication of Lord Rayleigh's recent results, Messrs. Glazebrook, Dodds, and Sargant, of Cambridge, communicated to the Royal Society two determinations of the Ohm, by different methods; and it is satisfactory to find that their final values differ only in the fourth decimal, the figures being, according to

Lord Rayleigh . . . $1 \text{ Ohm} = 0{\cdot}98651 \dfrac{\text{Earth Quadrant}}{\text{Second}}$

Messrs. Glazebrook, etc. $= 0{\cdot}986439$,,

Professor E. Wiedemann, of Leipzig, has lately called attention to the importance of having the Ohm determined in the most accurate manner possible, and enumerates four distinct methods, all of which should unquestionably be tried with a view of obtaining concordant results, because upon its accuracy will depend the whole future system of measurement of energy of whatever form.

The word Energy was first used by Young in a scientific sense, and represents a conception of recent date, being the outcome of the labours of Carnot, Mayer, Joule, Grove, Clausius, Clerk-Maxwell, Thomson, Stokes, Helmholtz, Macquorn-Rankine, and other labourers, who have accomplished for the science regarding the forces in Nature what we owe to Lavoisier, Dalton, Berzelius, Liebig, and others, as regards Chemistry. In this short word Energy we find all the efforts in nature, including electricity, heat, light, chemical action, and dynamics, equally represented, forming, to use Dr. Tyndall's apt expression, so many "modes of motion." It will readily be conceived that when we have established a fixed numerical relation between these different modes of motion, we know beforehand what is the utmost result we can possibly attain in converting one form of energy into another, and to what extent our apparatus for effecting the conversion falls short of realising it. The difference between ultimate theoretical effect and that actually obtained is commonly called loss, but, considering that energy is indestructible, represents really secondary effect which we obtain without desiring it. Thus friction in the working parts of a machine represents a loss of mechanical effect, but is a gain of heat, and in like manner the loss sustained in transferring electrical energy from one point to another is accounted for by heat generated in the conductor. It sometimes suits our purpose to augment the transformation of electrical into heat energy at certain points of the circuit when the heat rays become visible, and we have the incandescence electric light. In effecting a complete severance of the conductor for a short distance, after the current has been established, a very great local resistance is occasioned, giving rise to the electric arc, the highest development of heat ever attained. Vibration is another form of lost energy in mechanism, but who would call it a loss if it proceeded from the violin of a Joachim or a Norman-Neruda?

Electricity is the form of energy best suited for transmitting an effect from one place to another; the electric current passes through certain substances—the metals—with a velocity limited only by the retarding influence caused by electric charge of the surrounding dielectric, but approaching probably under favourable conditions that of radiant heat and light, or 300,000 kilometres per second; it refuses, however, to pass through oxidised substances, glass, gums, or through gases except when in a highly rarefied condition. It is easy therefore to confine the electric current within bounds, and to direct it through narrow channels of extraordinary length. The conducting wire of an Atlantic cable is such a narrow channel; it consists of a copper wire, or strand of wires, 5 mm. in diameter, by nearly 5,000 kilometres in length, confined electrically by a coating of gutta-percha about 4 mm. in thickness. The electricity from a small galvanic battery passing into this channel prefers the long journey to America in the good conductor, and back through the earth, to the shorter journey across the 4 mm. in thickness of insulating material. By an improved arrangement the alternating currents employed to work long submarine cables do not actually complete the circuit, but are merged in a condenser at the receiving station after having produced their extremely slight but certain effect upon the receiving instrument, the beautiful syphon recorder of Sir William Thomson. So perfect is the channel and so precise the action of both the transmitting and receiving instruments employed, that two systems of electric signals may be passed simultaneously through the same cable in opposite directions, producing independent records at either end. By the application of this duplex mode of working to the Direct United States cable under the superintendence of Dr. Muirhead, its transmitting power was increased from twenty-five to sixty words a minute, being equivalent to about twelve currents or primary impulses per second. In transmitting these impulse-currents simultaneously from both ends of the line, it must not be imagined, however, that they pass each other in the manner of liquid waves belonging to separate systems; such a supposition would involve momentum in the electric flow, and although the effect produced is analogous to such an action, it rests upon totally different grounds—namely, that of a local circuit at each terminus being called into action automatically whenever two similar currents are passed into the line simultaneously from both ends. In extending this principle of action quadruplex telegraphy has been rendered possible, although not yet for long submarine lines.

The minute currents here employed are far surpassed as regards delicacy and frequency by those revealed to us by that marvel of the present day, the telephone. The electric currents caused by the vibrations of a diaphragm acted upon by the human voice, naturally vary in frequency and intensity according to the number and degree of those vibrations, and each motor current in exciting the electro-magnet forming part of the receiving instrument, deflects the iron diaphragm occupying the position of an armature to a greater or smaller extent according to its strength. Savart found that the fundamental *la* springs from 440 complete vibrations in a second, but what must be the frequency and modulations of the motor current and of magnetic variations necessary to convey to the ear through the medium of a vibrating armature, such a complex of human voices and of musical instruments as constitutes an opera performance. And yet such performances could be distinctly heard and even enjoyed as an artistic treat by applying to the ears a pair of the double telephonic receivers at the Paris Electrical Exhibition, when connected with a pair of transmitting instruments in front of the footlights of the Grand Opera. In connection with the telephone, and with its equally remarkable adjunct the microphone, the names of Riess, Graham Bell, Edison, and Hughes, will ever be remembered.

Considering the extreme delicacy of the currents working a telephone, it is obvious that those caused by induction from neighbouring telegraphic line wires would seriously interfere with the former, and mar the speech or other sounds produced through their action. To avoid such interference the telephone wires if suspended in the air require to be placed at some distance from telegraphic line wires, and to be supported by separate posts. Another way of neutralising interference consists in twisting two separately insulated telephone wires together, so as to form a strand, and in using the two conductors as a metallic circuit to the exclusion of the earth; the working current will, in that case, receive equal and opposite inductive influences, and will therefore remain unaffected by them. On the other hand two insulated wires instead of one are required for working one set of instruments; and a serious increase in the cost of installation is thus caused. To avoid this Mr. Jacob has lately suggested a plan of combining pairs of such metallic circuits again into separate working pairs, and these again with other working pairs, whereby the total number of telephones capable of being worked without interference is made to equal the total number of single wires employed. The working of telephones and telegraphs in metallic circuit has the further advantage that mutual volta induction between the outgoing and returning currents favours the transit, and neutralises on the other hand the retarding influence caused by charge in underground or submarine conductors. These conditions are particularly favourable to underground line wires, which possess other important advantages over the still prevailing overground system, in that they are unaffected by atmospheric electricity, or by snow-storms and heavy gales, which at not very rare intervals of time put us back to pre-telegraphic days, when the letter-carrier was our swiftest messenger.

The underground system of telegraphs, first introduced into Germany by Werner Siemens in the years 1847-8, had to yield for a time to the overground system owing to technical difficulties, but it has been again resorted to within the last four years, and multiple land cables of solid construction now connect all the important towns of that country. The first cost of such a system is no doubt considerable (being about 38*l.* per kilometre of conductor as against 8*l.* 10*s.* the cost of land lines); but as the underground wires are exempt from frequent repairs and renewals, and as they insure continuity of service, they are decidedly the cheaper and better in the end. The experience afforded by the early introduction of the underground system in Germany, was not, however, without its beneficial results, as it brought to light the phenomena of lateral induction, and of faults in the insulating coating, matters which had to be understood before submarine telegraphy could be attempted with any reasonable prospect of success.

Regarding the transmission of power to a distance the electric current has now entered the lists in competition with compressed air, the hydraulic accumulator, and the quick running rope as used at Schaffhausen to utilise the power of the Rhine fall. The transformation of electrical into mechanical energy, can be accomplished with no further loss than is due to such incidental causes as friction and the heating of wires; these in a properly designed dynamo-electric machine do not exceed 10 per cent., as shown by Dr. John Hopkinson, and, judging from recent experiments of my own, a still nearer approach to ultimate perfection is attainable. Adhering, however, to Dr. Hopkinson's determination for safety's sake, and assuming the same percentage in reconverting the current into mechanical effect, a total loss of 19 per cent. results. To this loss must be added that through electrical resistance in the connecting line wires, which depends upon their length and conductivity, and that due to heating by friction of the working parts of the machine. Taking these as being equal to the internal losses incurred in the double process of conversion, there remains a useful effect of 100 — 38 = 62 per cent., attainable at a distance, which agrees with experimental results, although in actual practice it would not be safe at present to expect more than 50 per cent. of ultimate useful effect, to allow for all mechanical losses.

In using compressed air or water for the transmission of power the loss cannot be taken at less than 50 per cent., and as it depends upon fluid resistance it increases with distance more rapidly than in the case of electricity. Taking the loss of effect in all cases as 50 per cent., electric transmission presents the advantage that an insulated wire does the work of a pipe capable of withstanding high internal pressure, which latter must be more costly to put down and to maintain. A second metallic conductor is required, however, to complete the electrical circuit, as the conducting power of the earth alone is found unreliable for passing quantity currents, owing to the effects of polarization; but as this second conductor need not be insulated, water or gas pipes, railway metals or fencing wire, may be called into requisition for the purpose. The small space occupied by the electro-motor, its high working speed, and the absence of waste products, render it specially available for the general distribution of power to cranes and light machinery of every description. A loss of effect of 50 per cent. does not stand in the way of such applications, for it must be remembered that a powerful central engine of best construction produces motive power with a consumption of two pounds of coal per horse-power per hour, whereas small engines distributed over a district would consume not less than five; we thus see that there is an advantage in favour of electric transmission as regards fuel, independently of the saving of labour and other collateral benefits.

To agriculture, electric transmission of power seems well adapted for effecting the various operations of the farm and fields from one centre. Having worked such a system myself in combination with electric lighting and horticulture for upwards of two years, I can speak with confidence of its economy, and of the facility with which the work is accomplished in charge of untrained persons.

As regards the effect of the electric light upon vegetation there is little to add to what was stated in my paper read before Section A last year, and ordered to be printed with the Report, except that in experimenting upon wheat, barley, oats, and other cereals sown in the open air, there was a marked difference between the growth of the plants influenced and those uninfluenced by the electric light. This was not very apparent till towards the end of February, when, with the first appearance of mild weather, the plants under the influence of an electric lamp of 4,000 candle power placed about 5 metres above the surface, developed with extreme rapidity, so that by the end of May they stood 4 feet high, with the ears in full bloom, when those not under its influence were under 2 feet in height, and showed no sign of the ear.

In the electric railway first constructed by Dr. Werner Siemens, at Berlin, in 1879, electric energy was transmitted to the moving carriage or train of carriages through the two rails upon which it moved, these being sufficiently insulated from each other by being placed upon well creosoted cross sleepers. At the Paris Electrical Exhibition the current was conveyed through two separate conductors making sliding or rolling contact with the carriage, whereas in the electric railway now in course of construction in the north of Ireland (which when completed will have a length of twelve miles) a separate conductor will be provided by the side of the railway, and the return circuit completed through the rails themselves, which in that case need not be insulated; secondary batteries will be used to store the surplus energy created in running downhill, to be restored in ascending steep inclines, and for passing roadways where the separate insulated conductor is not practicable. The electric railway possesses great advantages over horse or steam-power for towns, in tunnels, and in all cases where natural sources of energy, such as waterfalls are available; but it would not be reasonable to suppose that it will in its present condition compete with steam propulsion upon ordinary railways. The transmission of power by means of electric conductors possesses the further advantage over other means of transmission that, provided the resistance of the rails be not very great, the power communicated to the locomotive reaches its maximum when the motion is at its minimum—that is, in commencing to work, or when encountering an exceptional resistance—whereas the utmost economy is produced in the normal condition of working when the velocity of the power-absorbing nearly equals that of the current-producing machine.

The deposition of metals from their solutions is perhaps the oldest of all useful applications of the electric current, but it is only in very recent times that the dynamo current has been practically applied to the refining of copper and other metals, as now practised at Birmingham and elsewhere, and upon an exceptionally large scale at Ocker, in Germany. The dynamo machine there employed was exhibited at the Paris Electrical Exhibition by Dr. Werner Siemens, its peculiar feature being that the conductors upon the rotating armature consisted of solid bars of copper 30 mm. square, in section, which were found only just sufficient to transmit the large quantity of electricity of low tension necessary for this operation. One such machine consuming 4-horse power deposits about 300 kilogrammes of copper per 24 hours; the motive power at Ocker is derived from a waterfall.

Electric energy may also be employed for heating purposes, but in this case it would obviously be impossible for it to compete in point of economy with the direct combustion of fuel for the attainment of ordinary degrees of heat. Bunsen and St. Claire Deville have taught us, however, that combustion becomes extremely sluggish when a temperature of 1,800° C. has been reached, and for effects at temperatures exceeding that limit the electric furnace will probably find advantageous applications. Its specific advantage consists in being apparently unlimited in the degree of heat attainable, thus opening out a new field of investigation to the chemist and metallurgist. Tungsten has been melted in such a furnace, and 8 pounds of platinum have been reduced from the cold to the liquid condition in 20 minutes.

The largest and most extensive application of electric energy at the present time is to lighting, but, considering how much of late has been said and written for and against this new illuminant, I shall here confine myself to a few general remarks. Joule has shown that if an electric current is passed through a conductor, the whole of the energy lost by the current is converted into heat; or, if the resistance be localised, into radiant energy comprising heat, light, and actinic rays. Neither the low heat rays nor the ultra-violet of highest refrangibility affect the retina, and may be regarded as lost energy, the effective rays being those between the red and violet of the spectrum, which in their combination produce the effect of white light.

Regarding the proportion of luminous to non-luminous rays proceeding from an electric arc or incandescent wire, we have a most valuable investigation by Dr. Tyndall, recorded in his work on "Radiant Heat." Dr. Tyndall shows that the luminous rays from a platinum wire heated to its highest point of incandescence, which may be taken at 1,700° C., formed $\frac{1}{24}$th part of the total radiant energy emitted, and $\frac{1}{10}$th part in the case of an arc light worked by a battery of 50 Grove's elements. In order to apply these valuable data to the case of electric lighting by means of dynamo-currents, it is necessary in the first place to determine what is the power of 50 Grove's elements of the size used by Dr. Tyndall, expressed in the practical scale of units as now established. From a few experiments lately undertaken for myself, it would appear that 50 such cells have an electro-motive force of 98·5 Volts, and an internal resistance of 13·5 Ohms, giving a current of 7·3 Ampères when the cells are short-circuited. The resistance of a regulator such as Dr. Tyndall used in his experiments may be taken at 10 Ohms, the current produced in the arc would be $\frac{98·5}{13·5 + 10 + 1} = 4$ Ampères (allowing one Ohm for the leads), and the power consumed $10 \times 4^2 = 160$ Watts; the light power of such an arc would be about 150 candles, and, comparing this with an arc of 3,308 candles produced by 1,162 Watts, we find that $\left(\frac{1162}{160}\right)$, *i.e.*, 7·3 times the electric energy produce $\left(\frac{3308}{150}\right)$, *i.e.*, 22 times the amount of light measured horizontally. If therefore, in Dr. Tyndall's arc $\frac{1}{10}$th of the radiant energy emitted was visible as light, it follows that in a powerful arc of 3,300 candles, $\frac{1}{10} \times \frac{22·0}{7·3}$, or fully $\frac{1}{3}$, are luminous rays. In the case of the incandescence light (say a Swan light of 20 candle power) we find in practice that nine times as much power has to be expended as in the case of the arc light; hence $\frac{1}{3} \times \frac{1}{9} = \frac{1}{27}$ part of the power is given out as luminous rays, as against $\frac{1}{24}$th in Dr. Tyndall's incandescent platinum—a result sufficiently approximate considering the wide difference of conditions under which the two are compared.

These results are not only of obvious practical value, but they seem to establish a fixed relation between current, temperature, and light produced, which may serve as a means to determine temperatures exceeding the melting point of platinum with greater accuracy than has hitherto been possible by actinimetric methods in which the thickness of the luminous atmosphere must necessarily exercise a disturbing influence. It is probably owing to this circumstance that the temperature of the electric arc as well as that of the solar photosphere has frequently been greatly over-estimated.

The principal argument in favour of the electric light is furnished by its immunity from products of combustion which not only heat the lighted apartments, but substitute carbonic acid and deleterious sulphur compounds for the oxygen upon which respiration depends; the electric light is white instead of yellow, and thus enables us to see pictures, furniture, and flowers as by daylight; it supports growing plants instead of poisoning them, and by its means we can carry on photography and many other industries at night as well as during the day. The objection frequently urged against the electric light, that it depends upon the continuous motion of steam or gas engines, which are liable to accidental stoppage, has been removed by the introduction into practical use of the secondary battery; this, although not embodying a new conception, has lately been greatly improved in power and constancy by Planté, Faure, Volckmar, Sellon, and others, and promises to accomplish for electricity what the gas-holder has done for the supply of gas, and the accumulator for hydraulic transmission of power.

It can no longer be a matter of reasonable doubt, therefore, that electric lighting will take its place as a public illuminant, and that even though its cost should be found greater than that of gas, it will be preferred for the lighting of drawing-rooms and dining-rooms, theatres and concert-rooms, museums, churches, warehouses, show-rooms, printing establishments and factories, and also the cabins and engine-rooms of passenger steamers. In the cheaper and more powerful form of the arc light, it has proved itself superior to any other illuminant for spreading artificial daylight over the large areas of harbours, railway-stations, and the sites of public works. When placed within a holophote the electric lamp has already become a powerful auxiliary in effecting military operations both by sea and land.

The electric light may be worked by natural sources of power such as waterfalls, the tidal wave, or the wind, and it is conceivable that these may be utilised at considerable distances by means of metallic conductors. Some five years ago I called attention to the vastness of those sources of energy, and the facility offered by electrical conduction in rendering them available for lighting and power supply, while Sir William Thomson made this important matter the subject of his admirable address to section A last year at York, and dealt with it in an exhaustive manner.

The advantages of the electric light and of the distribution of power by electricity have lately been recognised by the British Government, who have just passed a Bill through Parliament to facilitate the establishment of electrical conductors in towns, subject to certain regulating clauses to protect the interests of the public and of local authorities. Assuming the cost of electric light to be practically the same as gas, the preference for one or other will in each application be decided upon grounds of relative convenience, but I venture to think that gas-lighting will hold its own as the poor man's friend.

Gas is an institution of the utmost value to the artisan; it requires hardly any attention, is supplied upon regulated terms, and gives with what should be a cheerful light a genial warmth, which often saves the lighting of a fire. The time is moreover not far distant, I venture to think, when both rich and poor will largely resort to gas as the most convenient, the cleanest, and the cheapest of heating agents, and when raw coal will be seen only at the colliery or the gasworks. In all cases where the town to be supplied is within say thirty miles of the colliery, the gasworks may with advantage be planted at the mouth, or still better at the bottom of the pit, whereby all haulage of fuel would be avoided, and the gas, in its ascent from the bottom of the colliery, would acquire an onward pressure sufficient probably to impel it to its destination. The possibility of transporting combustible gas through pipes for such a distance has been proved at Pittsburg, where natural gas from the oil district is used in large quantities.

The quasi monopoly so long enjoyed by gas companies has had the inevitable effect of checking progress. The gas being supplied by meter, it has been seemingly to the advantage of the companies to give merely the prescribed illuminating power, and to discourage the invention of economical burners, in order that the consumption might reach a maximum. The application of gas for heating purposes has not been encouraged, and is still made difficult in consequence of the objectionable practice of reducing the pressure in the mains during daytime to the lowest possible point consistent with prevention of atmospheric indraught. The introduction of the electric light has convinced gas managers and directors that such a policy is no longer tenable, but must give way to one of technical progress; new processes for cheapening the production and increasing the purity and illuminating power of gas are being fully discussed before the Gas Institute; and improved burners, rivalling the electric light in brilliancy, greet our eyes as we pass along our principal thoroughfares.

Regarding the importance of the gas supply as it exists at present, we find from a Government return that the capital invested in gasworks in England, other than those of local authorities, amounts to 30,000,000*l.*; in these 4,281,048 tons of coal are converted annually, producing 43,000 million cubic feet of gas, and about 2,800,000 tons of coke; whereas the total amount of coal annually converted in the United Kingdom may be estimated at 9,000,000 tons, and the by-products therefrom at 500,000 tons of tar, 1,000,000 tons of ammonia liquor, and 4,000,000 tons of coke, according to the returns kindly furnished me by the managers of many of the gasworks and corporations. To these may be added say 120,000 tons of sulphur, which up to the present time is a waste product.

Previous to the year 1856—that is to say, before Mr. W. H. Perkin had invented his practical process, based chiefly upon the theoretical investigations of Hoffman, regarding the coal-tar bases and the chemical constitution of indigo—the value of coal-tar in London was scarcely a halfpenny a gallon, and in country places gas-makers were glad to give it away. Up to that time the coal-tar industry had consisted chiefly in separating the tar by distillation into naphtha, creosote, oils, and pitch. A few distillers, however, made small quantities of benzene, which had been first shown—by Mansfield, in 1849—to exist in coal-tar naphtha mixed with toluene, cumene, &c. The discovery, in 1856, of the mauve or aniline purple gave a great impetus to the coal-tar trade, inasmuch as it necessitated the separation of large quantities of benzene, or a mixture of benzene and toluene, from the naphtha. The trade was further increased by the discovery of the magenta or rosaniline dye, which required the same products for its preparation. In the meantime, carbolic acid was gradually introduced into commerce, chiefly as a disinfectant, but also for the production of colouring matter.

The next most important development arose from the discovery by Græbe and Liebermann that alizarine, the colouring principle of the madder root, was allied to anthracene, a hydrocarbon existing in coal-tar. The production of this colouring matter from anthracene followed, and is now one of the most important operations connected with tar distilling. The success of the alizarine made in this manner has been so great that it has almost entirely superseded the use of madder, which is now cultivated to only a comparatively small extent. The most important colouring matters recently introduced are the azo-scarlets. They have called into use the coal-tar hydrocarbons, xylene and cumene. Naphthalene is also used in their preparation. These splendid dyes have replaced cochineal in many of its applications, and have thus seriously interfered with its use. The discovery of artificial indigo by Professor Baeyer is of great interest. For the preparation of this colouring matter toluene is required. At present artificial indigo does not compete seriously with the natural product; but should it eventually be prepared in quantity from toluene, a further stimulus will be given to the coal-tar trade.

The colour industry utilises even now practically all the benzene, a large proportion of the solvent naptha, all the anthracene, and a portion of the napthaline resulting from the distillation of coal-tar; and the value of the colouring matter thus produced is estimated by Mr. Perkin at 3,350,000*l.*

The demand for ammonia may be taken as unlimited, on account of its high agricultural value as a manure; and, considering the failing supply of guano and the growing necessity for stimulating the fertility of our soil, an increased production of ammonia may be regarded as a matter of national importance, for the supply of which we have to look almost exclusively to our gasworks. The present production of 1,000,000 tons of liquor yields 95,000 tons of sulphate of ammonia; which, taken at 20*l.* 10*s.* a ton, represents an annual value of 1,947,000*l.*

The total annual value of the gasworks by-products may be estimated as follows:—

Colouring matter	£3,350,000
Sulphate of ammonia	1,947,000
Pitch (325,000 tons)	365,000
Creosote (25,000,000 gallons) . . .	208,000
Crude carbolic acid (1,000,000 gallons). .	100,000
Gas coke, 4,000,000 tons (after allowing 2,000,000 tons consumption in working the retorts) at 12*s.*	2,400,000
Total	£8,370,000

Taking the coal used, 9,000,000 tons, at 12*s.*, equal 5,400,000*l.*, it follows that the by-products exceed in value the coal used by very nearly 3,000,000*l.*

In using raw coal for heating purposes these valuable products are not only absolutely lost to us, but in their stead we are favoured with those semi-gaseous by-products in the atmosphere too well known to the denizens of London and other large towns as smoke. Professor Roberts has calculated that the soot in the pall hanging over London on a winter's day amounts to fifty tons, and that the carbonic oxide, a poisonous compound, resulting from the imperfect combustion of coal, may be taken as at least five times that amount. Mr. Aitken has shown, moreover, in an interesting paper communicated to the Royal Society of Edinburgh, last year, that the fine dust resulting from the imperfect combustion of coal is mainly instrumental in the formation of fog; each particle of solid matter attracting to itself aqueous vapour, these globules of fog are rendered particularly tenacious and disagreeable by the presence of tar vapour, another result of imperfect combustion of raw fuel, which might be turned to much better account at the dye-works. The hurtful influence of smoke upon public health, the great personal discomfort to which it gives rise, and the vast expense it indirectly causes through the destruction of our monuments, pictures, furniture, and apparel, are now being recognised, as is evinced by the success of recent Smoke Abatement Exhibitions. The most effectual remedy would result from a general recognition of the fact that wherever smoke is produced, fuel is being consumed wastefully, and that all our calorific effects, from the largest down to the domestic fire, can be realised as completely and more economically, without allowing any of the fuel employed to reach the atmosphere unburnt. This most desirable result may be effected by the use of gas for all heating purposes with or without the addition of coke or anthracite.

The cheapest form of gas is that obtained through the entire distillation of fuel in such gas producers as are now largely used in working the furnaces of glass, iron, and steel works; but gas of this description would not be available for the supply of towns owing to its bulk, about two-thirds of its volume being nitrogen. The use of water-gas, resulting from the decomposition of steam in passing through a hot chamber filled with coke, has been suggested, but this gas also is objectionable, because it contains, besides hydrogen, the poisonous and inodorous gas carbonic oxide, the introduction of which into dwelling-houses could not be effected without considerable danger. A more satisfactory mode of supplying heating separately from illuminating gas would consist in connecting the retort at different periods of the distillation with two separate systems of mains for the delivery of the respective gases. Experiments made some years ago by Mr. Ellisen of the Paris gasworks have shown that the gases rich in carbon, such as olefiant and acetylene, are developed chiefly during an interval of time, beginning half an hour after the commencement and terminating at half the whole period of distillation, whilst during the remainder of the time, marsh gas and hydrogen are chiefly developed, which, while possessing little illuminating power are most advantageous for heating purposes, By resorting to improved means of heating the retorts with gaseous fuel, such as have been in use at the Paris gasworks for a considerable number of years, the length of time for effecting each distillation may be shortened from six hours, the usual period in former years, to four, or even three hours, as now practised at Glasgow and elsewhere. By this means a given number of retorts can be made to produce, in addition to the former quantity of illuminating gas of superior quality, a similar quantity of heating gas, resulting in a diminished cost of production and an increased supply of the valuable by-products previously referred to. The quantity of both ammonia and heating gas may be further increased by the simple expedient of passing a streamlet of steam through the heated retorts towards the end of each operation, whereby the ammonia and hydrocarbons still occluded in the heated coke will be evolved, and the volume of heating gas produced be augmented by the products of decomposition of the steam itself. It has been shown that gas may be used advantageously for domestic purposes with judicious management even under present conditions, and it is easy to conceive that its consumption for heating would soon increase, perhaps tenfold, if supplied separately at say 1*s.* a thousand cubic feet. At this price gas would be not only the cleanest and most convenient, but also the cheapest form of fuel, and the enormous increase of consumption, the superior quality of the illuminating gas obtained by selection, and the proportionate increase of by-products, would amply compensate the gas company or corporation for the comparatively low price of the heating gas.

The greater efficiency of gas as a fuel results chiefly from the circumstance that a pound of gas yields in combustion 22,000 heat units, or exactly double the heat produced in the combustion of a pound of ordinary coal. This extra heating power is due partly to the freedom of the gas from earthy constituents, but chiefly to the heat imparted to it in effecting its distillation. Recent experiments with gas-burners have shown that in this direction also there is much room for improvement.

The amount of light given out by a gas flame depends upon the temperature to which the particles of solid carbon in the flame are raised, and Dr. Tyndall has shown that of the radiant energy set up in such a flame, only the $\frac{1}{25}$th part is luminous; the hot products of combustion carry off at least four times as much energy as is radiated, so that not more than one hundredth part of the heat evolved in combustion is converted into light. This proportion could be improved, however, by increasing the temperature of combustion, which may be effected either by intensified air currents or by regenerative action. Supposing that the heat of the products of combustion could be communicated to metallic surfaces, and be transferred by conduction or otherwise to the atmospheric air supporting combustion in the flame, we should be able to increase the temperature accumulatively to any point within the limit of dissociation; this limit may be fixed at about 2,300° C., and cannot be very much below that of the electric arc. At such a temperature the proportion of luminous rays to the total heat produced in combustion would be more than doubled, and the brilliancy of the light would at the same time be greatly increased. Thus improved, gas-lighting may continue its rivalry with electric lighting both as regards economy and brilliancy, and such rivalry must necessarily result in great public advantage.

In the domestic grate radiant energy of inferior intensity is required, and I for one do not agree with those who would like to see the open fireplace of this country, superseded by the continental stove. The advantages usually claimed for the open fireplace are, that it is cheerful, "pokable," and conducive to ventilation, but to these may be added another of even greater importance, viz., that the radiant heat which it emits passes through the transparent air without warming it, and imparts heat only to the solid walls, floor, and furniture of the room, which are thus constituted the heating surfaces of the comparatively cool air of the apartments in contact with them. In the case of stoves the heated air of the room causes deposit of moisture upon the walls in heating them, and gives rise to mildew and germs injurious to health. It is, I think, owing to this circumstance that upon entering an apartment one can immediately perceive whether or not it is heated by an open fireplace; nor is the unpleasant sensation due to stove-heating completely removed by mechanical ventilation; there is, moreover, no good reason why an open fireplace should not be made as economical and smokeless as a stove or hot-water apparatus.

In the production of mechanical effect from heat, gaseous fuel also presents most striking advantages, as will appear from the following consideration. When we have to deal with the question of converting mechanical into electrical effect, or *vice versâ*, by means of the dynamo-electrical machine, we have only to consider what are the equivalent values of the two forms of energy, and what precautions are necessary to avoid losses by the electrical resistance of conductors and by friction. The transformation of mechanical effect into heat involves no losses except those resulting from imperfect installation, and these may be so completely avoided that Dr. Joule was able by this method to determine the equivalent values of the two forms of energy. But in attempting the inverse operation of effecting the conversion of heat into mechanical energy, we find ourselves confronted by the second law of thermo-dynamics, which says that whenever a given amount of heat is converted into mechanical effect, another but variable amount descends from a higher to a lower potential, and is thus rendered unavailable.

In the condensing steam engine this waste heat comprises that communicated to the condensing water, whilst the useful heat, or that converted into mechanical effect, depends upon the difference of temperature between the boiler and condenser. The boiler pressure is limited, however, by considerations of safety and convenience of construction, and the range of working temperature rarely exceeds 120° C. except in the engines constructed by Mr. Perkins, in which a range of 160° C., or an expansive action commencing at 14 atmospheres, has been adopted with considerable promise of success, as appears from an able report on this engine by Sir Frederick Bramwell. To obtain more advantageous primary conditions we have to turn to the caloric or gas engine, because in them the co-efficient of efficiency expressed by $\frac{T-T'}{T}$ may be greatly increased. This value would reach a maximum if the initial absolute temperature T could be raised to that of combustion, and T' reduced to atmospheric temperature, and these maximum limits can be much more nearly approached in the gas engine worked by a combustible mixture of air and hydro-carbons than in the steam engine.

Assuming, then, in an explosive gas-engine a temperature of 1,500° C. at a pressure of 4 atmospheres, we should, in accordance with the second law of thermo-dynamics, find a temperature after expansion to atmospheric pressure of 600° C., and therefore a working range of $1500° - 600° = 900°$, and a theoretical efficiency $\frac{900}{1500 + 274}$ = about one-half, contrasting very favourably with that of a good expansive condensing steam-engine, in which the range is $150 - 30 = 120°$ C., and the efficiency $\frac{120}{150 + 274} = \frac{2}{7}$. A good expansive steam-engine is therefore capable of yielding as mechanical work $\frac{2}{7}$th part of the heat communicated to the boiler, which does not include the heat lost by imperfect combustion, and that carried away in the chimney. Adding to these, the losses by friction and radiation in the engine, we find that the best steam-engine yet constructed does not yield in mechanical effect more than $\frac{1}{7}$th part of the heat energy residing in the fuel consumed. In the gas-engine we have also to make reductions from the theoretical efficiency, on account of the rather serious loss of heat by absorption into the working cylinder, which has to be cooled artificially in order to keep its temperature down to a point at which lubrication is possible; this, together with frictional loss, cannot be taken at less than one-half, and reduces the factor of efficiency of the engine to $\frac{1}{4}$th.

It follows from these considerations that the gas or caloric engine combines the conditions most favourable to the attainment of maximum results, and it may reasonably be supposed that the difficulties still in the way of their application on a large scale will gradually be removed. Before many years have elapsed we may find in our factories and on board our ships engines with a fuel consumption not exceeding one pound of coal per effective horse power per hour, in which the gas producer takes the place of the somewhat complex and dangerous steam boiler. The advent of such an engine and of the dynamo-machine must mark a new era of material progress at least equal to that produced by the introduction of steam power in the early part of our century. Let us consider what would be the probable effect of such an engine upon that most important interest of this country—the merchant navy.

According to returns kindly furnished by the Board of Trade and *Lloyds' Register of Shipping*, the total value of the merchant shipping of the United Kingdom may be estimated at 126,000,000*l*., of which 90,000,000*l*. represents steamer having a net tonnage of 3,003,988 tons; and 36,000,000*l*. sailing vessels, of 3,688,008 tons. The safety of this vast amount of shipping, carrying about five-sevenths of our total imports and exports, or 500,000,000*l*. of goods in the year, and of the more precious lives connected with it, is a question of paramount importance. It involves considerations of the most varied kind: comprising the construction of the vessel itself, and the material employed in building it; its furniture of engines, pumps, sails, tackle, compass, sextant, and sounding apparatus, the preparation of reliable charts for the guidance of the navigator, and the construction of harbours of refuge, lighthouses, beacons, bells, and buoys, for channel navigation. Yet notwithstanding the combined efforts of science, inventive skill, and practical experience—the accumulation of centuries—we are startled with statements to the effect that during last year as many as 1,007 British owned ships were lost, of which fully two-thirds were wrecked upon our shores, representing a total value of nearly 10,000,000*l*. Of these ships 870 were sailing vessels and 137 steamers, the loss of the latter being in a fourth of the cases attributable to collision. The number of sailing vessels included in these returns being 19,325, and of steamers 5,505, it appears that the steamer is the safer vessel, in the proportion of 4·43 to 3·46; but the steamer makes on an average three voyage for one of the sailing ship taken over the year, which reduces the relative risk of the steamer as compared with the sailing ship per voyage in the proportion of 13·29 to 3·46. Commercially speaking, this factor of safety in favour of steam-shipping is to a great extent counterbalanced by the value of the steamship, which bears to that of the sailing vessel per net carrying ton the proportion of 3 : 1, thus reducing the ratio in favour of steam shipping as 13·29 to 10·38, or in round numbers as 4 : 3. In testing this result by the charges of premium for insurance, the variable circumstances of distance, nature of cargo, season and voyage have to be taken into account; but judging from information received from shipowners and underwriters of undoubted authority, I find that the relative insurance paid for the two classes of vessel represents an average of 30 per cent. in favour of steam shipping, agreeing very closely with the above deductions derived from statistical information.

In considering the question how the advantages thus established in favour of steam-shipping could be further improved, attention should be called in the first place to the material employed in their construction. A new material was introduced for this purpose by the Admiralty in 1876—78, when they constructed at Pembroke dockyard the two steam corvettes, the *Iris* and *Mercury*, of mild steel. The peculiar qualities of this material are such as to have enabled shipbuilders to save 20 per cent. in the weight of the ship's hull, and to increase to that extent its carrying capacity. It combines with a strength 30 per cent. superior to that of iron such extreme toughness, that in the case of collision the side of the vessel has been found to yield or bulge several feet without showing any sign of rupture, a quality affecting the question of sea risk very favourably. When to the use of this material there are added the advantages derived from a double bottom, and from the division of the ship's hold by means of bulkheads of solid construction, it is difficult to conceive how such a vessel could perish by collision either with another vessel or with a sunken rock. The spaces between the two bottoms are not lost, because they form convenient chambers for water ballast, but powerful pumps should in all cases be added to meet emergencies.

The following statement of the number and tonnage of vessels building and preparing to be built in the United Kingdom on the 30th of June last, which has been kindly furnished me by Lloyd's, is of interest as showing that wooden ships are fast becoming obsolete, and that even iron is beginning to yield its place, both as regards steamers and sailing ships, to the new material mild steel; it also shows that by far the greater number of vessels now building are ships of large dimensions propelled by engine power:—

	MILD STEEL.		IRON.		WOOD.		TOTAL.	
	No.	Tons gross.	No.	Tons gross.	No.	Tons gross.	No.	Tons gross.
Steam	89	159,751 ...	555	929,921 ...	6	460 ...	650	1,090.132
Sailing	11	16,800 ...	70	120,259 ...	49	4,635 ...	130	141,694
	100	176,551 ...	625	1,050,180 ...	55	5,095 ...	780	1,231,826

If to the improvements already achieved could be added an engine of half the weight of the present steam engine and boilers, and working with only half the present expenditure of fuel, a further addition of 30 per cent. could be made to the cargo of an Atlantic propeller vessel—no longer to be called a steamer—and the balance of advantages in favour of such vessels would be sufficient to restrict the use of sailing craft chiefly to the regattas of this and neighbouring ports.

The admirable work on the "British Navy," lately published by Sir Thomas Brassey, the Civil Chief Lord of the Admiralty, shows that the naval department of this country is fully alive to all improvements having regard to the safety as well as to the fighting qualities of Her Majesty's ships of war, and recent experience goes far to prove that although high speed and manœuvring qualities are of the utmost value, the armour plate which appeared to be fast sinking in public favour is not without its value in actual warfare.

The progressive views perceptible in the construction of the navy are further evidenced in a remarkable degree in the hydrographic department. Captain Sir Frederick Evans, the hydrographer, and Vice-President of the British Association, gave us at York last year a very interesting account of the progress made in that department, which, while dealing chiefly with the preparation of charts showing the depth of water, the direction and force of currents, and the rise of tides near our shores, contains also valuable statistical information regarding the more general questions of the physical conditions of the sea, its temperature at various depths, its flora and fauna, as also the rainfall and the nature and force of prevailing winds. In connection with this subject the American Naval Department has taken an important part, under the guidance of Captain Maury and the Agassiz father and son, whilst in this country the persistent labours of Dr. William Carpenter deserve the highest consideration.

Our knowledge of tidal action has received a most powerful impulse through the invention of a self-recording gauge and tide-predicter, which will form the subject of one of the discourses to be delivered at our present meeting by its principal originator, Sir William Thomson; when I hope he will furnish us with an explanation of some extraordinary irregularities in tidal records, observed some years ago by Sir John Coode at Portland, and due apparently to atmospheric influence.

The application of iron and steel in naval construction rendered the use of the compass for some time illusory, but in 1839 Sir George Airy showed how the errors of the compass due to the influence experienced from the iron of the ship, may be perfectly corrected by magnets and soft iron placed in the neighbourhood of the binnacle, but the great size of the needles in the ordinary compasses rendered the correction of the quadrantal errors practically unattainable. In 1876 Sir William Thomson invented a compass with much smaller needles than those previously used, which allows Sir George Airy's principles to be applied completely. With this compass correctors can be arranged so that the needle shall point accurately in all directions, and these correctors can be adjusted at sea from time to time, so as to eliminate any error which may arise through change in the ship's magnetism or in the magnetism induced by the earth through change of the ship's position. By giving the compass card a long period of free oscillation great steadiness is obtained when the ship is rolling.

Sir William Thomson has also enriched the art of navigation by the invention of two sounding machines; the one being devised for ascertaining great depths very accurately in less than one-quarter the time formerly necessary, and the other for taking depths up to 130 fathoms without stopping the ship in its onward course. In both these instruments steel pianoforte wire is used instead of the hempen and silken lines formerly employed; in the latter machine the record of depth is obtained not by the quantity of wire run over its counter and brake wheel, but through the indications produced upon a simple pressure gauge consisting of an inverted glass tube, whose internal surface is covered beforehand with a preparation of chromate of silver rendered colourless by the sea-water up to the height to which it penetrates. The value of this instrument for guiding the navigator within what he calls "soundings" can hardly be exaggerated; with the sounding machine and a good chart he can generally make out his position correctly by a succession of three or four casts in a given direction at given intervals, and thus in foggy weather is made independent of astronomical observations and of the sight of lighthouses or the shore. By the proper use of this apparatus, such accidents as happened to the mail steamer *Mosel* not a fortnight ago would not be possible. As regards the value of the deep-sea instrument I can speak from personal experience, as on one occasion it enabled those in charge of the Cable s.s. *Faraday* to find the end of an Atlantic Cable, which had parted in a gale of wind, with no other indication of the locality than a single sounding, giving a depth of 950 fathoms. To recover the cable a number of soundings in the supposed neighbourhood of the broken end were taken, the 950 fathom contour line was then traced upon a chart, and the vessel thereupon trailed its grapnel two miles to the eastward of this line, when it soon engaged the cable 20 miles away from the point, where dead reckoning had placed the ruptured end.

Whether or not it will ever be practicable to determine oceanic depths without a sounding line, by means of an instrument based upon gravimetric differences, remains to be seen. Hitherto the indications obtained by such an instrument have been encouraging, but its delicacy has been such as to unfit it for ordinary use on board a ship when rolling.

The time allowed me for addressing you on this occasion is wholly insufficient to do justice to the great engineering works of the present day, and I must therefore limit myself to making a short allusion to a few only of the more remarkable enterprises.

The great success, both technically and commercially, of the Suez Canal, has stimulated M. de Lesseps to undertake a similar work of even more gigantic proportions, namely, the piercing of the Isthmus of Panama by a ship canal, 40 miles long, 50 yards wide on the surface, and 20 yards at the bottom, upon a dead level from sea to sea. The estimated cost of this work is 20,000,000*l*., and more than this sum having been subscribed, it appears unlikely that political or climatic difficulties will stop

M. de Lesseps in its speedy accomplishment. Through it, China, Japan, and the whole of the Pacific Ocean will be brought to half their present distance, as measured by the length of voyage, and an impulse to navigation and to progress will thus be given which it will be difficult to over-estimate.

Side by side with this gigantic work, Captain Eads, the successful improver of the Mississippi navigation, intends to erect his ship-railway, to take the largest vessels, fully laden and equipped, from sea to sea, over a gigantic railway across the Isthmus of Tehuantepec, a distance of ninety-five miles. Mr. Barnaby, the chief constructor of the navy, and Mr. John Fowler have expressed a favourable opinion regarding this enterprise, and it is to be hoped that both the canal and the ship-railway will be accomplished, as it may be safely anticipated that the traffic will be amply sufficient to support both these undertakings.

Whether or not M. de Lesseps will be successful also in carrying into effect the third great enterprise with which his name has been prominently connected, the flooding of the Tunis-Algerian Chotts, thereby re-establishing the Lake Tritonis of the ancients, with its verdure-clad shores, is a question which could only be decided upon the evidence of accurate surveys, but the beneficial influence of a large sheet of water within the African desert could hardly be matter of doubt.

It is with a feeling not unmixed with regret that I have to record the completion of a new Eddystone Lighthouse in substitution for the *chef-d'œuvre* of engineering erected by John Smeaton more than 100 years ago. The condemnation of that structure was not, however, the consequence of any fault of construction, but was caused by inroads of the sea upon the rock supporting it. The new lighthouse, designed and executed by Mr., now Sir James Douglas, engineer of Trinity House, has been erected in the incredibly short time of less than two years, and bids fair to be worthy of its famed predecessor. Its height above high water is 130 feet, as compared with 72 feet, the height of Telford's structure, which gives its powerful light a considerably increased range. The system originally suggested by Sir William Thomson some years ago, of distinguishing one light from another by flashes following at varied intervals, has been adopted by the Elder Brethren in this as in other recent lights in the modified form introduced by Dr. John Hopkinson, in which the principle is applied to revolving lights, so as to obtain a greater amount of light in the flash.

The geological difficulties which for sometime threatened the accomplishment of the St. Gothard Tunnel, have been happily overcome, and this second and most important sub-Alpine thoroughfare now connects the Italian railway system with that of Switzerland and the south of Germany, whereby Genoa will be constituted the shipping port for those parts.

Whether we shall be able to connect the English with the French railway system by means of a tunnel below the English Channel is a question that appears dependent at this moment rather upon military and political than technical and financial considerations. The occurrence of a stratum of impervious grey chalk, at a convenient depth below the bed of the Channel, minimises the engineering difficulties in the way, and must influence the financial question involved. The protest lately raised against its accomplishment can hardly be looked upon as a public verdict, but seems to be the result of a natural desire to pause pending the institution of careful inquiries. These inquiries have been made by a Royal Scientific Commission, and will be referred for further consideration to a mixed Parliamentary Committee, upon whose Report it must depend whether the natural spirit of commercial enterprise has to yield in this instance to political and military considerations. Whether the Channel Tunnel is constructed or not, the plan proposed some years ago by Mr. John Fowler of connecting England and France by means of a ferry boat capable of taking railway trains would be a desideratum justified by the ever-increasing intercommunication between this and Continental countries.

The public inconvenience arising through the obstruction to traffic by a sheet of water is well illustrated by the circumstance that both the estuaries of the Severn and of the Mersey are being undermined in order to connect the railway systems on the two sides, and that the Frith of Forth is about to be spanned by a bridge exceeding in grandeur anything as yet attempted by the engineer. The roadway of this bridge will stand 150 feet above high-water mark, and its two principal spans will measure a third of a statute mile each. Messrs. Fowler and Baker, the engineers to whom this great work has been entrusted, could hardly have accomplished their task without having recourse to steel for their material of construction, nor need the steel used be of the extra mild quality particularly applicable for naval structures to withstand collision, for, when such extreme toughness is not required, steel of very homogeneous quality can be produced, bearing a tensile strain double that of iron.

The tensile strength of steel, as is well known, is the result of an admixture of carbon with the iron, varying between $\frac{1}{10}$th and 2 per cent., and the nature of this combination of carbon with iron is a matter of great interest both from a theoretical and practical point of view. It could not be a chemical compound which would necessitate a definite proportion, nor could a mere dissolution of the one in the other exercise such remarkable influence upon the strength and hardness of the resulting metal. A recent investigation by Mr. Abel has thrown considerable light upon this question. A definite carbide of iron is formed, it appears, soluble at high temperatures in iron, but separating upon cooling the steel gradually, and influencing only to a moderate degree the physical properties of the metal as a whole. In cooling rapidly there is no time for the carbide to separate from the iron, and the metal is thus rendered both hard and brittle. Cooling the metal gradually under the influence of great compressive force, appears to have a similar effect to rapid cooling in preventing the separation of the carbide from the metal, with this difference, that the effect is more equal throughout the mass, and that more uniform temper is likely to result.

When the British Association met at Southampton on a former occasion, Schönbein announced to the world his discovery of gun-cotton. This discovery has led the way to many valuable researches on explosives generally, in which Mr. Abel has taken a leading part. Recent investigations by him, in connection with Captain Noble, upon the explosive action of gun-cotton and gunpowder confined in a strong chamber, which have not yet been published, deserve particular attention. They show that while by the method of investigation pursued about twenty years ago by Karolye (of exploding gunpowder in very small charges in shells confined within a large shell partially exhausted of air), the composition of the gaseous products was found to be complicated and liable to variation, the chemical metamorphosis which gun-cotton sustains, when exploded under conditions such as obtain in its practical application, is simple and very uniform. Among other interesting points noticed in this direction was the fact that, as in the case of gunpowder, the proportion of carbonic acid increases, while that of carbonic oxide diminishes with the density of the charge. The explosion of gun-cotton, whether in the form of wool or loosely spun thread, or in the packed compressed form devised by Abel, furnished practically the same results if fired under pressure, that is, under strong confinement—the conditions being favourable to the full development of its explosive force; but some marked differences in the composition of the products of metamorphosis were observed when gun-cotton was fired by detonation. With regard to the tension exerted by the products of explosion, some interesting points were observed, which introduce very considerable difficulties into the investigation of the action of fired gun-cotton. Thus whereas no marked differences are observed in the tension developed by small charges and by very much larger charges of gunpowder having the same density (*i.e.*, occupying the same volume relatively to the entire space in which they are exploded) the reverse is the case with respect to gun-cotton. Under similar conditions in regard to density of charge, 100 grammes of gun-cotton gave a measured tension of about 20 tons on the square inch, 1,500 grammes gave a tension of about 29 tons (in several very concordant observations), while a charge of 2·5 kilos gave a pressure of about 45 tons, this being the maximum measured tension obtained with a charge of gunpowder of five times the density of the above.

The extreme violence of the explosion of gun cotton as compared with gunpowder when fired in a closed space was a feature attended with formidable difficulties. In whatever way the charge was arranged in the firing cylinder, if it had free access to the enclosed crusher gauge, the pressures recorded by the latter were always much greater than when means were taken to prevent the wave of matter suddenly set in motion from acting directly upon the gauge. The abnormal or wave-pressures recorded at the same time that the general tension in the cylinder was measured amounted in the experiment to 42·3 tons, when the general tension was recorded at 20 tons; and in another when the pressure was measured at 29 tons, the wave-pressure recorded was 44 tons. Measurements of the temperature of explosion of gun-cotton showed it to be about double that of the explosion of gunpowder. One of the effects observed to be produced by this sudden enormous development of heat was the covering of the inner surfaces of the steel explosion-vessel with a net-work of cracks, small portions of the surface being sometimes actually fractured. The explosion of charges of gun-cotton up to 2·5 kilos in perfectly closed chambers, with development of pressures approaching to 50 tons on the square inch, constitutes alone a perfectly novel feat in investigations of this class.

Messrs. Noble and Abel are also continuing their researches upon fired gunpowder, being at present occupied with an inquiry into the influence exerted upon the chemical metamorphosis and ballistic effects of fired gunpowder by variation in its composition, their attention being directed especially to the discovery of the cause of the more or less considerable erosion of the interior surface of guns produced by the exploding charge—an effect which, notwithstanding the application of devices in the building up of the charge specially directed to the preservation of the gun's bore, have become so serious that, with the enormous charges now used in our heavy guns, the erosive action on the surface of the bore produced by a single round is distinctly perceptible. As there appeared to be *primâ facie* reasons why the erosive action of powder upon the surface of the bore at the high temperatures developed should be at any rate in part due to its one component sulphur, Noble and Abel have made comparative experiments with powders of usual composition and with others in which the proportion of sulphur was considerably increased, the extent of erosive action of the products escaping from the explosion vessel under high tension being carefully determined. With small charges a particular powder containing no sulphur was found to exert very little erosive action as compared with ordinary cannon powder; but another powder, containing the maximum proportion of sulphur tried (15 per cent.), was found equal to it under these conditions. and exerted very decidedly less erosive action than it, when larger charges were reached. Other important contributions to our knowledge of the action of fired gunpowder in guns, as well as decided improvements in the gunpowder manufactured for the very heavy ordnance of the present day, may be expected to result from a continuance of these investigations. Prof. Carl Himly, of Kiel, having been engaged upon investigations of a similar nature, has lately proposed a gunpowder in which hydrocarbons precipitated from solution in naphtha take the place of the charcoal and sulphur of ordinary powder, this powder has amongst others the peculiar property of completely resisting the action of water, so that the old caution, "Keep your powder dry," may hereafter be unnecessary.

The extraordinary difference of condition, before and after its ignition, of such matter as constitutes an explosive agent leads us up to a consideration of the aggregate state of matter under other circumstances. As early as 1776 Alexander Volta observed that the volume of glass was changed under the influence of electrification, by what he termed electrical pressure. Dr. Kerr, Govi, and others have followed up the same inquiry, which is at present continued chiefly by Dr. George Quincke, of Heidelberg, who finds that temperature, as well as chemical constitution of the dielectric under examination, exercises a determining influence upon the amount and character of the change of volume effected by electrification; that the change of volume may under certain circumstances be effected instantaneously as in flint glass, or only slowly as in crown glass, and that the elastic limit of both is diminished by electrification, whereas in the case of mica and of guttapercha an increase of elasticity takes place.

Still greater strides are being made at the present time towards a clearer perception of the condition of matter when particles are left some liberty to obey individually the forces brought to bear upon them. By the discharge of high tension electricity through tubes containing highly rarefied gases (Geissler's tubes), phenomena of discharge were produced which were at once most striking and suggestive. The Sprengel pump afforded a means of pushing the exhaustion to limits which had formerly been scarcely reached by the imagination. At each step the condition of attenuated matter revealed varying properties when acted upon by electrical discharge and magnetic force. The radiometer of Crookes imported a new feature into these inquiries, which at the present time occupy the attention of leading physicists in all countries.

The means usually employed to produce electrical discharge in vacuum tubes was Ruhmkorff's coil; but Mr. Gassiot first succeeded in obtaining the phenomena by means of a galvanic battery of 3000 Leclanché cells. Dr. De La Rue, in conjunction with his friend Dr. Hugo Müller, has gone far beyond his predecessors in the production of batteries of high potential. At his lecture "On the phenomena of electrical discharge," delivered at the Royal Institution in January 1881, he employed a battery of his invention consisting of 14,400 cells (14,832 Volts), which gave a current of 0·054 Ampère, and produced a discharge at a distance of 0·71 inch between the terminals. During last year he increased the number of cells to 15,000 (15,450 Volts), and increased the current to 0·4 Ampère or eight times that of the battery he used at the Royal Institution.

With the enormous potential and perfectly steady current at his disposal, Mr. De La Rue has been able to contribute many interesting facts to the science of electricity. He has shown, for example, that the beautiful phenomena of the stratified discharge in exhausted tubes are but a modification and a magnification of those of the electric arc at ordinary atmospheric pressure. Photography was used in his experiments to record the appearance of the discharge, so as to give a degree of precision otherwise unattainable in the comparison of the phenomena. He has shown that between two points the length of the spark, provided the insulation of the battery is efficacious, is as the square of the number of cells employed. Mr. De La Rue's experiments have proved that at all pressures the discharge in gases is not a current in the ordinary acceptation of the term, but is of the nature of a disruptive discharge. Even in an apparently perfectly steady discharge in a vacuum tube, when the strata as seen in a rapidly revolving mirror are immovable, he has shown that the discharge is a pulsating one; but, of course, the period must be of a very high order.

At the Royal Institution, on the occasion of his lecture, Mr. De La Rue produced, in a very large vacuum tube, an imitation of the aurora borealis; and he has deduced from his experiments that the greatest brilliancy of aurora displays must be at an altitude of from thirty-seven to thirty-eight miles—a conclusion of the highest interest, and in opposition to the extravagant estimate of 281 miles at which it had been previously put.

The President of the Royal Society has made the phenomena of electrical discharge his study for several years, and resorted in his important experiments to a special source of electric power. In a note addressed to me, Dr. Spottiswoode describes the nature of his investigations much more clearly than I could venture to give them. He says: "It had long been my opinion that the dissymmetry shown in electrical discharges through rarefied gases must be an essential element of every disruptive discharge, and that the phenomena of stratification might be regarded as magnified images of features always present, but concealed under ordinary circumstances. It was with a view to the study of this question that the researches by Moulton and myself were undertaken. The method chiefly used consisted in introducing into the circuit intermittence of a particular kind, whereby one luminous discharge was rendered sensitive to the approach of a conductor outside the tube. The application of this method enabled us to produce artificially a variety of phenomena, including that of stratification. We were thus led to a series of conclusions relating to the mechanism of the discharge, among which the following may be mentioned:—

1. That a stria, with its attendant dark space, forms a physical unit of a striated discharge; that a striated column is an aggregate of such units formed by means of a step-by-step process; and that the negative glow is merely a localised stria, modified by local circumstances.

2. That the origin of the luminous column is to be sought for at its negative end; that the luminosity is an expression of a demand for negative electricity; and that the dark spaces are those regions where the negative terminal, whether metallic or gaseous, is capable of exerting sufficient influence to prevent such demand.

3. That the time occupied by electricity of either name in traversing a tube is greater than that occupied in traversing an equal length of wire, but less than that occupied by molecular streams (Crooke's radiations) in traversing the tubes. Also that, especially in high vacua, the discharge from the negative terminal exhibits a durational character not found at the positive.

4. That the brilliancy of the light with so little heat may be due in part to brevity in the duration of the discharge; and that for action so rapid as that of individual discharges, the mobility of the medium may count as nothing; and that for these infini-

tesimal periods of time gas may itself be as rigid and as brittle as glass.

5. That striæ are not merely loci in which electrical is converted into luminous energy, but are actual aggregations of matter.

This last conclusion was based mainly upon experiments made with an induction coil excited in a new way—viz. directly by an alternating machine, without the intervention of a commutator or condenser. This mode of excitement promises to be one of great importance in spectroscopic work, as well as in the study of the discharge in a magnetic field, partly on account of the simplification which it permits in the construction of induction coils, but mainly on account of the very great increase of strength in the secondary currents to which it gives rise.

These investigations assume additional importance when we view them in connection with solar—I may even say stellar—physics, for evidence is augmenting in favour of the view that interstellar space is not empty, but is filled with highly attenuated matter of a nature such as may be put into our vacuum tubes. Nor can the matter occupying stellar space be said any longer to be beyond our reach for chemical and physical test. The spectroscope has already thrown a flood of light upon the chemical constitution and physical condition of the sun, the stars, the comets, and the far distant nebulæ, which have yielded spectroscopic photographs under the skilful management of Dr. Huggins, and Dr. Draper of New York. Armed with greatly improved apparatus, the physical astronomer has been able to reap a rich harvest of scientific information during the short periods of the last two solar eclipses—that of 1879, visible in America, and that of May last, observed in Egypt by Lockyer, Schuster, and by Continental observers of high standing. The result of this last eclipse expedition has been summed up as follows:—"Different temperature levels have been discovered in the solar atmosphere; the constitution of the corona has now the possibility of being determined, and it is proved to shine with its own light. A suspicion has been aroused once more as to the existence of a lunar atmosphere, and the position of an important line has been discovered. Hydro-carbons do not exist close to the sun, but may in space between us and it."

To me personally these reported results possess peculiar interest, for in March last I ventured to bring before the Royal Society a speculation regarding the conservation of solar energy, which was based upon the three following postulates, viz.:—

1. That aqueous vapour and carbon compounds are present in stellar or interplanetary space.

2. That these gaseous compounds are capable of being dissociated by radiant solar energy while in a state of extreme attenuation.

3. That the effect of solar rotation is to draw in dissociated vapours upon the polar surfaces, and to eject them after combustion has taken place back into space equatorially.

It is therefore a matter of peculiar gratification to me that the results of observation here recorded give considerable support to that speculation. The luminous equatorial extensions of the sun which the American observations revealed in such a striking manner (with which I was not acquainted when writing my paper) were absent in Egypt; but the outflowing equatorial streams I suppose to exist could only be rendered visible by reflected sunlight, when mixed with dust produced by exceptional solar disturbances or by electric discharge; and the occasional appearance of such luminous extensions would serve only to disprove the hypothesis entertained by some, that they are divided planetary matter, in which case their appearance should be permanent. Prof. Langley, of Pittsburg, has shown, by means of his bolometer, that the solar actinic rays are absorbed chiefly in the solar instead of in the terrestrial atmosphere, and Capt. Abney has found by his new photometric method that absorption due to hydrocarbons takes place somewhere between the solar and terrestrial atmosphere; in order to test this interesting result still further, he has lately taken his apparatus to the top of the Riffel with a view of diminishing the amount of terrestrial atmospheric air between it and the sun, and intends to bring a paper on this subject before Section A. Stellar space filled with such matter as hydrocarbon and aqueous vapour would establish a material continuity between the sun and his planets, and between the innumerable solar systems of which the universe is composed. If chemical action and reaction can further be admitted, we may be able to trace certain conditions of thermal dependence and maintenance, in which we may recognise principles of high perfection, applicable also to comparatively humble purposes of human life.

We shall thus find that in the great workshop of nature there are no lines of demarcation to be drawn between the most exalted speculation and common-place practice, and that all knowledge must lead up to one great result, that of an intelligent recognition of the Creator through His works. So then, we members of the British Association and fellow-workers in every branch of science may exhort one another in the words of the American bard who has so lately departed from amongst us:—

> "Let us then be up and doing,
> With a heart for any fate;
> Still achieving, still pursuing,
> Learn to labour and to wait."

SECTION A

MATHEMATICAL AND PHYSICAL

Opening Address by the Right Hon. Lord Rayleigh, M.A., F.R.S., F.R.A.S, President of the Section

In common with some of my predecessors in this chair, I recognise that probably the most useful form which a presidential address could take, would be a summary of the progress of physics, or of some important branch of physics, during recent years. But the difficulties of such a task are considerable, and I do not feel myself equal to grappling with them. The few remarks which I have to offer are of a general, I fear it may be thought of a commonplace character. All I can hope is that they may have the effect of leading us into a frame of mind suitable for the work that lies before us.

The diversity of the subjects which come under our notice in this section, as well as of the methods by which alone they can be adequately dealt with, although a sign of the importance of our work, is a source of considerable difficulty in the conduct of it. From the almost inevitable specialisation of modern science, it has come about that much that is familiar to one member of our section is unintelligible to another, and that details whose importance is obvious to the one fail altogether to rouse any interest in the mind of the other. I must appeal to the authors of papers to bear this difficulty in mind, and to confine within moderate limits their discussion of points of less general interest.

Even within the limits of those departments whose foundation is evidently experimental, there is room, and indeed necessity, for great variety of treatment. One class of investigators relies mainly upon reiterated appeals to experiment to resolve the questions which appear still to be open, while another prefers, with Thomas Young, to base its decisions as far as possible upon deductions from experiments already made by others. It is scarcely necessary to say that in the present state of science both methods are indispensable. Even where we may fairly suppose that the fundamental principles are well established, careful and often troublesome work is necessary to determine with accuracy the constants which enter into the expression of natural laws. In many cases the accuracy desirable, even from a practical point of view, is hard to attain. In many others, where the interest is mainly theoretical, we cannot afford to neglect the confirmations which our views may derive from the comparison of measurements made in different fields and in face of different experimental difficulties. Examples of the inter-dependence of measurements apparently distinct will occur to every physicist. I may mention the absolute determinations of electrical resistance, and of the amounts of heat developed from electrical and mechanical work, any two of which involve also the third, and the relation of the velocity of sound to the mechanical and thermal properties of air.

Where a measurement is isolated, and not likely to lead to the solution of any open question, it is doubtless possible to spend upon it time and attention that might with advantage be otherwise bestowed. In such a case we may be properly be satisfied for a time with work of a less severe and accurate character, knowing that with the progress of knowledge the way is sure to be smoothed both by a better appreciation of the difficulties involved, and by the invention of improved experimental appliances. I hope I shall not be misunderstood as underrating the importance of great accuracy in its proper place if I express the opinion that the desire for it has sometimes had a prejudicial effect. In cases where a rough result would have sufficed for all immediate purposes, no measurement at all has been attempted, because the circumstances rendered it unlikely that a high standard of precision could be attained. Whether our aim be more or less ambitious, it is important to recognise the limitations to which our methods are necessarily subject, and as far as possible to estimate the extent to which our results are uncertain. The comparison of estimates of uncertainty made before and after the execution of a set of measurements may sometimes be humiliating, but it is always instructive.

Even when our results show no greater discrepancies than we were originally prepared for, it is well to err on the side of modesty in estimating their trustworthiness. The history of science teaches only too plainly the lesson that no single method is absolutely to be relied upon, that sources of error lurk where they are least expected, and that they may escape the notice of the most experienced and conscientious worker. It is only by the concurrence of evidence of various kinds and from various sources that practical certainty may at last be attained, and complete confidence justified. Perhaps I may be allowed to illustrate my meaning by reference to a subject which has engaged a good deal of my attention for the last two years—the absolute measurement of electrical resistance. The unit commonly employed in this country is founded upon experiments made about twenty years ago by a distinguished committee of this Association, and was intended to represent an absolute resistance of 10^9. C.G.S., *i.e.* one ohm. The method employed by the committee at the recommendation of Sir W. Thomson (it had been originally proposed by Weber) consisted in observing the deflection from the magnetic meridian of a needle suspended at the centre of a coil of insulated wire, which formed a closed circuit, and was made to revolve with uniform and known speed about a vertical axis. From the speed and deflection in combination with the mean radius of the coil and the number of its turns, the absolute resistance of the coil, and thence of any other standard, can be determined.

About ten years later Kohlrausch attacked the problem by another method, which it would take too long to explain, and arrived at the result that the B.A. unit was equal to 1·02 ohms—about two per cent. too large. Rowland, in America, by a comparison between the steady battery current flowing in a primary coil with the transient current developed in a secondary coil when the primary current is reversed, found that the B.A. unit was ·991 ohms. Lorentz, using a different method again, found ·980, while H. Weber, from distinct experiments, arrived at the conclusion that the B.A. unit was correct. It will be seen that the results obtained by these highly competent observers range over about four per cent. Two new determinations have lately been made in the Cavendish laboratory at Cambridge, one by myself with the method of the revolving coil, and another by Mr. Glazebrook, who used a modification of the method followed by Rowland, with the result that the B.A. unit is ·986 ohms. I am now engaged upon a third determination, using a method which is a modification of that of Lorentz.

In another important part of the field of experimental science, where the subject-matter is ill understood, and the work is qualitative rather than quantitative, success depends more directly upon sagacity and genius. It must be admitted that much labour spent in this kind of work is ill-directed. Bulky records of crude and uninterpreted observations are not science, nor even in many cases the raw material out of which science will be constructed. The door of experiment stands always open; and when the question is ripe, and the man is found, he will nine times out of ten find it necessary to go through the work again. Observations made by the way, and under favourable conditions, may often give rise to valuable suggestions, but these must be tested by experiment, in which the conditions are simplified to the utmost, before they can lay claim to acceptance.

When an unexpected effect is observed, the question will arise whether or not an explanation can be found upon admitted principles. Sometimes the answer can be quickly given; but more often it will happen that an assertion of what *ought* to have been expected can only be made as the result of an elaborate discussion of the circumstances of the case, and this discussion must generally be mathematical in its spirit, if not in its form. In repeating, at the beginning of the century, the well-known experiment of the inaudibility of a bell rung *in vacuo*, Leslie made the interesting observation that the presence of hydrogen was inimical to the production of sound, so that not merely was the sound less in hydrogen than in air of equal pressure, but that the actual addition of hydrogen to rarefied air caused a diminution in the intensity of sound. How is this remarkable fact to be explained? Does it prove that, as Herschel was inclined to think, a mixture of gases of widely different densities differs in its acoustical properties from a single gas? These questions could scarcely be answered satisfactorily but by a mathematical investigation of the process by which vibrations are communicated from a vibrating solid body to the surrounding gas. Such an investigation, founded exclusively upon principles well established before the date of Leslie's observation, was undertaken years afterwards by Stokes, who proved that what Leslie observed was exactly what ought to have been expected. The addition of hydrogen to attenuated air increases the wave-length of vibrations of given pitch, and consequently the facility with which the gas can pass round the edge of the bell from the advancing to the retreating face, and thus escape those rarefactions and condensations which are essential to the formation of a complete sound wave. There remains no reason for supposing that the phenomenon depends upon any other elements than the density and pressure of the gaseous atmosphere, and a direct trial, *e.g.* a comparison between air and a mixture of carbonic anhydride and hydrogen of like density, is almost superfluous.

Examples such as this, which might be multiplied *ad libitum*, show how difficult it often is for an experimenter rightly to interpret his results without the aid of mathematics. It is eminently desirable that the experimenter himself should be in a position to make the calculations, to which his work gives occasion, and from which in return he would often receive valuable hints for further experiment. I should like to see a course of mathematical instruction arranged with especial reference to physics, within which those whose bent was plainly towards experiment might, more or less completely, confine themselves. Probably a year spent judiciously on such a course would do more to qualify the student for actual work than two or three years of the usual mathematical curriculum. On the other side, it must be remembered that the human mind is limited, and that few can carry the weight of a complete mathematical armament without some repression of their energies in other directions. With many of us difficulty of remembering, if not want of time for acquiring, would impose an early limit. Here, as elsewhere, the natural advantages of a division of labour will assert themselves. Innate dexterity and facility of contrivance, backed by unflinching perseverance, may often conduct to successful discovery or invention a man who has little taste for speculation; and on the other hand the mathematician, endowed with genius and insight, may find a sufficient field for his energies in interpreting and systematising the work of others.

The different habits of mind of the two schools of physicists sometimes lead them to the adoption of antagonistic views on doubtful and difficult questions. The tendency of the purely experimental school is to rely almost exclusively upon direct evidence, even when it is obviously imperfect, and to disregard arguments which they stigmatise as theoretical. The tendency of the mathematician is to overrate the solidity of his theoretical structures, and to forget the narrowness of the experimental foundation upon which many of them rest.

By direct observation, one of the most experienced and successful experimenters of the last generation convinced himself that light of definite refrangibility was capable of further analysis by absorption. It has happened to myself, in the course of measurements of the absorbing power of various media for the different rays of the spectrum, to come across appearances at first sight strongly confirmatory of Brewster's views, and I can therefore understand the persistency with which he retained his opinion. But the possibility of further analysis of light of definite refrangibility (except by polarisation) is almost irreconcilable with the wave theory, which on the strongest grounds had been already accepted by most of Brewster's contemporaries; and in consequence his results, though urgently pressed, failed to convince the scientific world. Further experiment has fully justified this scepticism, and in the hands of Airy, Helmholtz, and others, has shown that the phenomena by which Brewster was misled can be explained by the unrecognised intrusion of diffused light. The anomalies disappear when sufficient precaution is taken that the refrangibility of the light observed shall really be definite.

On similar grounds undulationists early arrived at the conviction that physically light and invisible radiant heat are both vibrations of the same kind, differing merely in wave-length; but this view appears to have been accepted slowly, and almost reluctantly, by the experimental school.

When the facts which appear to conflict with theory are well defined and lend themselves easily to experiment and repetition, there ought to be no great delay in arriving at a judgment.

Either the theory is upset, or the observations, if not altogether faulty, are found susceptible of another interpretation. The difficulty is greatest when the necessary conditions are uncertain, and their fulfilment rare and uncontrollable. In many such cases an attitude of reserve, in expectation of further evidence, is the only wise one. Premature judgments err perhaps as much on one side as on the other. Certainly in the past many extraordinary observations have met with an excessive incredulity. I may instance the fire-balls which sometimes occur during violent thunderstorms. When the telephone was first invented, the early reports of its performances were discredited by many on quite insufficient grounds.

It would be interesting, but too difficult and delicate a task, to enumerate and examine the various important questions which remain still undecided from the opposition of direct and indirect evidence. Merely as illustrations I will mention one or two in which I happen to have been interested. It has been sought to remedy the inconvenience caused by excessive reveberation of sound in cathedrals and other large unfurnished buildings by stretching wires overhead from one wall to another. In some cases no difference has been perceived, but in others it is thought that advantage has been gained. From a theoretical point of view it is difficult to believe that the wires could be of service. It is known that the vibrations of a wire do not communicate themselves in any appreciable degree directly to the air, but require the intervention of a sounding-board, from which we may infer that vibrations in the air would not readily communicate themselves to stretched wires. It seems more likely that the advantage supposed to have been gained in a few cases is imaginary than that the wires should really have played the part attributed to them.

The other subject on which, though with diffidence, I should like to make a remark or two, is that of Prout's law, according to which the atomic weights of the elements, or at any rate of many of them, stand in simple relation to that of hydrogen. Some chemists have reprobated strongly the importation of *a priori* views into the consideration of the question, and maintain that the only numbers worthy of recognition are the immediate results of experiment. Others, more impressed by the argument that the close approximations to simple numbers cannot be merely fortuitous, and more alive to the inevitable imperfections of our measurements, consider that the experimental evidence against the simple numbers is of a very slender character, balanced, if not outweighed, by the *a priori* argument in favour of simplicity. The subject is eminently one for further experiment; and as it is now engaging the attention of chemists, we may look forward to the settlement of the question by the present generation. The time has perhaps come when a redetermination of the densities of the principal gases may be desirable—an undertaking for which I have made some preparations.

If there is any truth in the views that I have been endeavouring to impress, our meetings in this section are amply justified. If the progress of science demands the comparison of evidence drawn from different sources, and fully appreciated only by minds of different order, what may we not gain from the opportunities here given for public discussion, and, perhaps more valuable still, private interchange of opinion? Let us endeavour, one and all, to turn them to the best account.

SECTION B

CHEMICAL SCIENCE

Opening Address by Prof. G. D. Liveing, M.A., F.R.S., F.C.S., President of the Section

If I were asked in what direction chemical science had of late been making the most important advance, I should reply that it was in the attempt to place the dynamics of chemistry on a satisfactory basis, to render an account of the various phenomena of chemical action on the same mechanical principles as are acknowledged to be true in other branches of physics. I cannot say that chemistry can yet be reckoned amongst what are called the exact sciences, that the results of bringing together given matters under given circumstances can yet be deduced in more than a few special cases by mere mathematical processes from mechanical principles, but that some noteworthy advances have in recent years been made which seem to bring such a solution of chemical problems more nearly within our reach.

To show how large a gap in our ideas of chemical dynamics has been bridged over within the last quarter of a century, I will quote the words of one of the largest-minded philosophers of his time, who was one of the earliest promoters of this Association, and its President in 1841; Whewell, in a new and much altered edition of his "Philosophy of the Inductive Sciences," published in 1858, says:—"Since Newton's time the use of the word *attraction* as expressing the cause of the union of the chemical elements of bodies has been familiarly continued; and has no doubt been accompanied in the minds of many persons with an obscure notion that chemical attraction is in some way a kind of mechanical attraction of the particles of bodies. Yet the doctrine that *chemical* attraction and *mechanical* attraction are forces of the same kind, has never, so far as I am aware, been worked out into a system of chemical theory; nor even applied with any distinctness as an explanation of any particular chemical phenomena. Any such attempt, indeed, could only tend to bring more clearly into view the entire inadequacy of such a mode of explanation. For the leading phenomena of chemistry are all of such a nature that no mechanical combination can serve to express them without an immense accumulation of additional hypotheses." And further on he says:—"We must consider the power which produces chemical combination as a peculiar principle, a special relation of the elements, not rightly expressed in mechanical terms." (Hist. of Scientific Ideas, II., pp. 13, 14).

The influence by which our ideas have gone round so as to be now the very opposite of those of the illustrious thinker whom I have just quoted, so that we should ridicule the thought of looking for an explanation of chemical action on any but mechanical principles, is undoubtedly the progress which has been made in other branches of molecular physics. The indestructibility of matter has long been a formula familiar to chemists, but that the conservation of energy should be as universally true even in regard to chemical actions, has only in recent years been fully recognised. This is certainly no new principle, it was developed mathematically generations ago; but the realisation that it is anything more than abstraction, that it is the keynote of every rational explanation of physical phenomena, has been the foundation of recent progress in physical science; and if all energy be one, there can be but one code of dynamical laws which must apply to chemistry as well as all other branches of physics. The development of the mechanical theory of heat, and of the molecular theories which have grown up in consequence of it, have done much to set our minds free from preconceived notions, and to induce us to build chemical theories on something more than unverified conjectures.

But how far can we say that mechanical principles are actually recognised as the true basis of rational chemistry? So far as I know no chemist denies that it is so, and yet how little do our text-books, even the most recent and the most highly reputed, show the predominance of this idea! How very small a portion of such books is taken up with it; how much seems utterly to ignore it, or to be couched in language which is antagonistic to it! We still find chemical combinations described as if they were statical phenomena, and expressions used which imply that two perfectly elastic bodies can by their mutual action alone bring each other into fixed relative positions. We still find change of valency described as a suppression of "bonds of affinity," as if a suppression of forces were the usual course of nature, or as it it were possible that the same two forces, acting at the same place and in the same direction, should at one time neutralise one another, and at another time not neutralise one another. We still find saturated compounds spoken of, as if the stability of a compound were independent of circumstances, and chemical combination no function of temperature and pressure. Beginners are sometimes helped by the invention of intermediate reactions in explanation of final results, without any reference to the dynamical conditions of the problem, without any consideration whether the fancied intermediate reactions imply a winding up or a running down of energy. In fact our long familiar chemical equations represent only the conservation of matter and to keep always in mind the mechanical conditions of a reaction is as difficult to some of us as it is to think in a foreign language. Moreover we still find in many of our text-books the old statical notion of chemical combination stereotyped in pictures of molecules. I do not, of course, mean to accuse the distinguished inventors of graphic formulæ of meaning to depict molecules, for I believe that they would agree with me in thinking that these diagrams do not any more nearly represent actual molecules than they represent the solar system; but unfortunately we cannot prevent beginners from regarding them as pictures, and moulding their ideas upon them. They present something easily grasped by the infant mind, and schoolmasters are fond of them; but only those who have each year to combat a fresh crop of misconceptions and false mechanical notions engendered by them, can be aware how much they hinder, I won't say the advance, but the spread of real chemical science. If it be true that the illustrations of an artist like the late Hablot Browne give to our conceptions of the characters of a story a more definite and permanent, though perhaps a much modified form of what the author of the story intended to portray, it is equally true that the illustrations by which some, even great names among us, have tried to make us fancy that we had a true conception of some natural process have become so fixed in our minds, as to prevent our realising the true meaning of nature.

What, then, is the progress which I think has been made in physical chemistry? In the first place, notwithstanding the slowness with which new ideas replace old familiar images, the molecular theories developed by Clausius, Clerk-Maxwell, Boltzmann, and by Sir W. Thomson, have been long enough before the world to have greatly loosened the hold upon our minds of many old notions. The rigid, unbreakable, impenetrable atoms of the Epicurean philosophy made familiar to us by Lucretius always presented difficulties which were only perhaps exceeded by those of the elastic atmospheres with which modern philosophers fancied them to be surrounded; but now the vortex theory, whether we think it probable or not, at least gives us a standing ground for the assertion that the supposed impenetrability of matter and the curious compound of nucleus and atmosphere which has been invented to account for elasticity are not necessary assumptions. The kinetic theory of gases has analysed for us the different motions of the molecules in a mass of matter, and has facilitated the conception of the part which heat plays in chemical action. Hence we have had of late several attempts to reduce to a form susceptible of mathematical calculation the problems of chemistry. Most of these attempts have proceeded on the well-known mechanical principle that the change of *vis viva* of a system in passing from an initial to a final configuration is independent of the intermediate stages through which it may have passed so long as the external conditions are unaltered; and on the principle of the dissipation of energy, that is to say, on the condition that the state of the system, if it be a stable one, must be such that the energy run down in reaching it is a maximum. These principles have been applied successfully to the solution of some particular cases of the equilibrium between a mixture of chemicals by Willard Gibbs, Berthelot, and others. By the first-mentioned principle, all consideration of the intermediate stages by which the final result is reached are avoided. Quite recently Lemoinne has attacked the same problem on another principle. His principle is that of an equilibrium of antagonistic reactions in a mixture of materials, a mobile equilibrium such as we are now familiar with, dependent on compensating effects; but he does not seem able to solve the problem in any great number of cases. In fact, the difficulty does not now lie so much in expressing mathematically the conditions of the problem as in the defect of knowledge which depends upon experiment. And it is just in this that I think the outlook most hopeful. In some cases the patient work of weighing and measuring and comparing, which is necessary to make our theoretic speculations of any substantial value, has been already done for us. The publication, three years since, of Berthelot's essay on chemical mechanics has given us in a collected form a large quantity of data of the first importance; and now I am glad to say that the long labours of another worker in the same field, Thomsen of Copenhagen, are in course of publication in a handy form. I think these two investigators have done more than any one else of late years towards making it possible to give to chemistry the rank of an exact science. But besides the data which they have supplied to us, there are others which are yet wanting. For instance, almost every equation of chemical equilibrium involves an expression depending on the specific heats of the materials. At present we do not know enongh of the law of specific heats to be able to give in most cases a probable value to those expressions; but these and other data of the kind do not seem out of our reach, and we may hope that the same ingenuity and patience which hss gained for us so much firm ground in thermal chemistry will extend it to the uncertain spots where we have yet no solid foundation.

Further, the laws of dissociation so ably investigated by Deville have taught us that the force called chemical affinity, by which we suppose the atoms of unlike matters are held together in a compound molecule, follows precisely the same laws as the force of cohesion, by which particles of a similar kind are united in molecules. We have long known that the molecules of sulphur vapour are broken up into simpler molecules by elevation of temperature, and condense again when the temperature is reduced. Other elementary substances behave in a similar way. We have within the last two or three years learnt that iodine is in part dissociated by a high temperature from molecules consisting of two chemical atoms into molecules consisting of only one such atom, and the same is true of chlorine and bromine. That some such change must occur in iodine and other metalloids was inferred as long ago as 1864 by the younger Mitscherlich. He argued that iodine is a compound body from the fact that it shows two spectra—one similar in character to those of metallic oxides, and the other similar to the spectra of metals; and from the analogy in the behaviour of iodine to a metallic oxide in giving the one spectrum at one temperature, and the other at a higher temperature, "from this it would follow that iodine at ordinary temperatures and iodine at the temperature of a hydrogen flame must be conceived as two different compounds, because the spectrum of iodine formed at ordinary temperatures" "*i.e.* the absorption spectrum of iodine vapour" "is different from that produced in a hydrogen flame. Also, "that bromine, though it gives no flame spectrum, gives one spectrum by absorption, and another by the electric spark, and must therefore in its ordinary state be regarded as a compound." Also that "the spectra formed by the flames of selenium, tellurium, and phosphorus, and those of sulphur and nitrogen given by feeble electric discharges, all have the character of the iodine flame spectrum, and these metalloids would therefore, if the above expressed supposition with regard to iodine be confirmed, also be compound bodies" (*Phil, Mag.*, 1864, p. 188). Since the paper from which the foregoing sentence is taken was published, not only the metalloids, but many metals have been found to give complicated spectra at one temperature, and much simpler spectra at higher temperatures. Such are the channelled spectra of sodium and potassium first described by Roscoe and Schuster, the channelled spectra of silver, bismuth, and other metals described by Lockyer and Roberts, and the ultra-violet channelled spectrum of tin recently photographed by Prof. Dewar and myself. But Mitscherlich's hypothesis gives us a rational explanation of such multiple spectra produced by the same substance, and it has been accepted in one form or another by all spectroscopists since he wrote.

Nevertheless, the existence of multiple spectra cannot be taken as a proof of allotropic modification, unless the possibility of a chemical combination is excluded. The channelled spectrum which magnesium gives in hydrogen was mistaken by more than one observer for that of some modification of the simple metal, until it was shown that magnesium in nitrogen and other gases does not give it, provided hydrogen be excluded, and that its persistence in hydrogen at high temperatures depends, as it should if due to a chemical combinations, on the pressure of the gas. If, however, homogeneous molecules are dissociated by heat, so also are heterogeneous molecules, formed as we say by chemical combination, split up by elevation of temperature, to unite again on cooling or by increase of pressure within certain limits. Nor is there any essential difference in character between a chemical compound and an element beyond that of facility of decomposition. If we could not so easily resolve them into their constituents, and were to disregard the characteristic differences of the spectra, no one would suppose ammonium to be constituted differently from potassium, or cyanogen from chlorine. Indeed, chemists have long been in the habit of considering the union of two atoms in a molecule of ordinary hydrogen or chlorine as a species of chemical combination, but when we find that the combinations of particles of the same kind are as definite as those of particles of different kinds, and that they are both subject to precisely the same mechanical laws, we are hardly justified in regarding the forces by which they are produced as essentially different. To get rid of a gratuitous hypothesis in chemistry must be a great gain.

But it may be asked why stop here? Why may not the chemical elements be further broken up by still higher temperatures? *A priori* and from analogy, such a supposition is extremely probable. The notion that there is but one elementary kind of matter is at least as old as Thales, and underlies Prout's hypothesis that the atomic weights of our elements are all multiples of that of hydrogen. This famous hypothesis has gone up and down in the scale of credibility many times during the present century. About seventeen years ago the publication of Stas' new determinations of combining numbers, carrried out on a scale never before attempted, and with all the refinements which the growth of our knowledge could suggest, was thought to have given it its deathblow. But a reaction has set in since that time. The periodic recurrence of the properties of elements with regular additions to the atomic weights, like octaves on a musical scale, put forcibly before us by Mendelejeff, makes it difficult not to think that there is a simple relation between the atomic weights, though there may be causes producing slight perturbations of such a relation. Quite recently a fresh revision of the combining weights has been made on the other side of the Atlantic by Prof. F. W. Clarke. He has collected all the determinations made by different observers, and after rejecting such as from defective methods were untrustworthy, has applied to the remainder such corrections as newer experiences have suggested, and then deduced from the corrected numbers the most probable values by the methods of the theory of errors. Prof. Clarke has done a piece of work of the highest utility, for which chemists must be grateful; nevertheless he has not carried the revision so far as it might be carried. He has, to begin with, rightly separated the several sets of observations, and deduced the most probable number for each set by itself, but in combining the various sets for the final determination of the numbers adopted, he has treated the results obtained by different methods as if they were a set of observations all presumably of equal value, so that the most probable numbers could be deduced by the method of least squares. He has not attempted any discussion of the different methods with a view to an estimate of the relative values of the results obtained by them, nor made any difference between the values of the figures deduced from operations on the large scale employed by Stas, and those arrived at on the small scales of other observers. Any sort of handicapping of methods is no doubt a very difficult and delicate operation, and requires more than the judgment of an Admiral Rous, but without it the question whether the numbers adopted are the best obtainable, will always be an open one. It is, however, a very noteworthy fact that in almost every case the numbers deduced from Stas' experiments taken by themselves, coincide very closely with the most probable numbers derived by the method of least squares from the whole of the recorded estimates. On the whole, Prof. Clarke concludes that Prout's hypothesis, as modified by Dumas, is still an open question; that is to say, his final numbers differ from whole multiples of a common unit by quantities which lie within the limits of errors of observation and experiment.

Let us turn again to the evidence afforded by our most powerful instrument for inspecting the inner constitution of matter, the spectroscope. A few years ago Mr. Lockyer supposed that the coincidence of rays emitted by different chemical elements, particularly when those rays were developed in the spark of a powerful induction-coil and in the high temperatures of the sun and stars, gave evidence of a common element in the composition of the metals which produced the coincident rays. Such an argument could not be drawn from the coincidences unless they were exact, and the identity of the lines could only be tested by means of spectroscopes of great resolving power. By the use of the well-known Rutherford gratings, Young, in America, had found that most of the solar lines which had been ascribed to two metals were in reality double, and Prof. Dewar and I, working on the terrestrial elements in the electric arc, had found the actual coincidences to be very few indeed. These observations, even with a Rutherford grating, were delicate enough; but quite recently M. Fievez, of the Brussels Observatory, has brought to bear on this question a spectroscope of unexampled power. By combining two of the Astronomer-Royal's highly-dispersive half-prisms with a Rutherford grating of 17,296 lines to the inch, he has obtained a dispersion quadruple that of Thollon's combination of prisms. Bringing this to bear on the sun, he has mapped the solar spectrum from a little below C to somewhere above F on a scale one-third greater than that of Vogel's map, and has not only confirmed the work of Young, Dewar, and myself, but has resolved some lines which were not divisible with such dispersive power as we had at command. This result cannot fail to shake our belief, if we have any, in the existence of any common constituent of the chemical elements; but it does not touch the evidence which the spectroscope affords us that many of our elements in the state in which we know them must have a very complex molecular structure. I cannot illustrate this point better than by the spectra of two of our commonest elements, magnesium and iron. We have good reason to think the molecule of magnesium to be as simple as that of any of our elements, and its spectrum is one of the simplest, consisting of a series of triplets which repeat each other in a regular way and are probably harmonically related, and of a comparatively small number of single lines, of which also some may be harmonics. The spectrum of iron, on the other hand, presents thousands of lines distributed irregularly through the whole length, not only of the visible, but of the ultra-violet region. Make what allowance you please for unknown harmonic relations and for lines not reversible, which may not be directly due to vibrations of the molecules, we still have a number of vibrations so immense that we can hardly conceive any single molecule capable of all of them, and are almost driven to suppose them to be due to a mixture of differing molecules, though as yet we have no independent evidence of this, and no satisfactory proof that any of this mixture is of the same kind as occurs in other elements.

M. Fievez's combination is a great advance in resolving power, but Prof. Rowland, of the Johns Hopkins University, promises us gratings not only exceeding Rutherford's, both in dimensions and accuracy of ruling, but ruled upon curved surfaces, so as to dispense with the use of telescopes and avoid all variations of focussing the different orders of spectra. His instruments, if they come up to the promise he holds out, will enable us to solve many questions which are difficult to answer with our present appliances.

But to return to the chemical elements: the spectroscope has in the last few years revealed to us several new metals. I will not venture to say how many; for when several new metals more or less closely allied are discovered at the same time, the process of sifting out their differences is necessarily a slow one. We cannot tell yet whether any of them are to fill gaps in Mendelejeff's table, and so add strength to the conviction that there is a natural relation between the atomic weights and the chemical characters of our elementary substances; or whether they will add to the embarrassment in which we already find ourselves with regard to the relations of the cerium group of metals; whether we may welcome them as the supporters of order, or deprecate their coming as authors of confusion. Granting that the chemical characters of an element are connected with its atomic weight, we have, however, no right to assume them to be dependent on that factor alone. Why may there not be elements which, while they differ as little in atomic weight as do nickel and cobalt, are, on the other hand, so similar to one another in all characters, that their chemical separation may be a matter of the greatest difficulty, and their difference only distinguishable by the spectroscope? The spectra may be thought to suggest so much, and how shall we decide the question? At any rate the complications of the spectroscopic problem are such as can only be unravelled by the united efforts of chemists and physicists, and by the exercise of extreme caution.

I cannot dismiss the subject of chemical dynamics without alluding to the ingenious theory by which the President of the Association has proposed to account for the conservation of solar energy. He supposes planetary space to be pervaded by an atmosphere which, except where it is condensed by the attraction of the sun and planets, is in a highly attenuated state. The sun and planets communicate some of their own motion of rotation to the atmosphere condensed about them, and he supposes that in this way an action like that of a blowing fan is set up, by which the equatoreal part of the sun's atmosphere acquires such a velocity as to stream out to distances beyond the earth's orbit, while an equal quantity of gas is drawn in at the poles to maintain equilibrium. The gases thus driven to a distance in planetary space will of course be enormously expanded and highly attenuated, and in this state Dr. Siemens thinks that such of them as are compound may be decomposed by absorbing the solar radiation, and thus the kinetic energy of solar radiation be converted into the potential energy of chemical separation. The separated elements or partial compounds will, in the circulation produced by the fanlike action of the solar rotation, be carried back to the polar regions of the sun as fuel to maintain his temperature by condensation and re-combination. I will not discuss the mechanical part of this theory farther than to remark that the fan-like action can only be carried on at the expense of the energy of the sun's rotation, which must be in consequence continually diminishing, and must in time become too slow to produce any sensible projection of the atmosphere into distant regions of planetary space. As to the chemical side of the theory, Dr. Siemens supposes the gases which pervade the planetary space to be not only of the same kind as the components of our own atmosphere, which, on the kinetic theory of gases diffuse through that space, but also such gases as are not found in our air, but are found occluded in meteorites which may be supposed to have acquired them in their previous wanderings. Amongst these he specially mentions hydrocarbons which form the self-luminous part of most comets. It is to these gases, together with aqueous vapour, and carbonic acid, that he ascribes the principal part in the conservation of solar energy. That compound gases at the extremely low pressure of the planetary space are decomposed by solar radiation is not inconsistent with the laws of dissociation, for it is quite possible that some compounds may be decomposed at ordinary temperatures by mere reduction of pressure, and the radiation absorbed will be the more effective, because it will directly affect the vibratory motion within the molecule, and may well produce chemical decomposition before it can, when the free path of the molecules is so much increased by the attenuation of the gas, assume the form of an increased temperature. Dr. Siemens, moreover, adduces a remarkable experiment in confirmation of his supposition. We know, too, the power which our atmosphere, and especially the water vapour in it, has of absorbing the infra-red rays, and that amongst the Fraunhofer lines some of the strongest groups are due to aqueous vapour, and the capital observation made by the spectroscopic observers at the last total eclipse, that the group of lines known as "B," which is one of those produced by aqueous vapour, is greatly strengthened when the sun's light passes by the edge of the moon and so through the lunar atmosphere, may be taken as a confirmation of the theory that gases, like our atmosphere, are diffused through space and concentrated about the planets. But if it be true that the compounds are decomposed by absorbing the sun's rays, we ought to find in our atmosphere the products of decomposition, we ought to find in it free hydrogen, carbonic oxide, and acetylene or some other hydrocarbons. The hydrogen from its small specific gravity would not be concentrated in the lower regions of our atmosphere in the same proportion as the denser gases, but carbonic oxide and hydrocarbons could not fail to be detected in the air if they formed any sensible proportion of the gases in the planetary space. That a large portion of the solar radiation is intercepted before it reaches the earth, is no doubt true, for there are not only the dark bands which are increased by our atmosphere, and may reasonably be attributed in part to the action of like gases pervading the space between us and the sun, but there is a continuous absorption of the ultra-violet spectrum beyond the line U, and Cornu has found that this absorption is not sensibly affected by our atmosphere, so that the substance, whatever it may be which produces it, may be an agent in the process imagined by Dr. Siemens, but cannot be the means of restoring to the sun any portion of the radiant energy which reaches our distance from him.

Dr. Siemens explains the self-luminous character of comets by the theory that the streams of meteoric stones, of which they are supposed to consist, bring from stellar space hydrocarbon and other gases occluded within them; and that in consequence of the rise of temperature due to the frictional resistance of such a divided mass moving with enormous velocity, aided by attractive condensation, the occluded gases will be driven out and burnt, the flame giving rise to the original light emitted by the nucleus. Now the spectrum of most comets shows only the principal bands of a Bunsen burner, and is therefore adequately explained by the flame of gas containing hydrocarbons, such as have been found in meteorites. But Dr. Huggins has observed in the spectrum of more than one comet not only hydrocarbon, but cyanogen bands, and, although carbon and nitrogen combine readily in the electric arc, a coal gas flame in air shows no trace of the spectrum of cyanogen, and it would certainly put some strain on our credulity if it were asserted that cyanogen were one of the gases brought ready-formed by meteorites from stellar space. Prof. Dewar and I have, however, recently shown that if nitrogen already in combination, as, for instance, in ammonia, be brought into a hydrocarbon flame, cyanogen is produced in sufficient amount to give in a photograph (but not so as to be directly visible) the characteristic spectrum of cyanogen as it appears in the comets. It is therefore no longer necessary to make any other supposition to account for the cyanogen bands in the spectra of comets than that ammonia or some such compound of nitrogen is present, as well as hydrocarbons in a state of ignition.

Quite recently Dr. Huggins has observed that the principal comet of this year has a spectrum of an entirely different character, but he is not yet able to say to what elements or compounds it is probably due. The notion that comets may bring us news of distant parts of stellar space, towards which our system is driving, where the atmosphere is not like ours, oxygen and nitrogen, but hydrogen and hydrocarbons, may fascinate the fancy, but the laws of occlusion oblige us to think that the meteorites have not merely wandered through an attenuated atmosphere of hydrogen and hydrocarbons, but have cooled in a much denser atmosphere of these substances, which we can only conceive as concentrated by the presence of a star or some large aggregation of matter. They may perchance have come from some nebulous mass, for Draper and Huggins tell us that in the great nebula in Orion, hydrogen is dense enough and hot enough to show some of its characteristic lines, besides the F line, which is seen in other nebulæ, and is the last to disappear by reduction of density. No comet on visiting our system a second time can repeat the exclusion of its occluded gases unless its store has been replenished in the interval, and it will be interesting to see when Halley's comet next returns, whether it shines only by reflected light, or gives us, like so many others, the banded spectrum of hydrocarbons.

SECTION D

BIOLOGY

OPENING ADDRESS BY ARTHUR GAMGEE, M.D., F.R.S., BRACKENBURY PROFESSOR OF PHYSIOLOGY IN OWENS COLLEGE, MANCHESTER, PRESIDENT OF THE SECTION.

On the Growth of our Knowledge of the Function of Secretion, to which is prefixed a Brief Sketch of the Writings of the late Professor Francis Maitland Balfour.—When the Council of the British Association did me the honour of asking me to preside over this section, it occurred to me that a suitable subject for the presidential address would be a Survey of the Growth of our Knowledge of the Function of Secretion; for no subject, which has recently been the object of minute study by animal physiologists, is more likely to interest all devoted to biological pursuits, however diverse. I accordingly propose to direct your attention, for the greater part of the time at our disposal to-day, to what appears to me to be the most important and the most interesting of the researches bearing on this subject.

Before, however, entering upon the proper subject of this address, it would ill become me as president of this section were I not to speak to you, however imperfectly, of two great losses which we have sustained, and which have saddened, and still sadden, the hearts of many of us. The year 1882 will long be memorable, and sadly memorable, as a year during which English biology sustained irreparable losses. So much has lately been written concerning that veteran in science, Charles Darwin, who will figure in the history of the human intellect with such men as Socrates and Newton, that I feel no words of mine are needed to add to your sentiments of admiration and respect. He has made for himself an imperishable reputation as one of the subtlest, most patient, and most truthful observers of natural phenomena. His powers as an observer were, however, almost surpassed by his ingenuity as a reasoner, and his power to frame the hypotheses most apt, in the actual state of science, to reconcile all the facts which came within the range of his observation. We remember the time when the name of Charles Darwin, and the mention of the theories connected with his name, awakened, on the part of many, sentiments of antagonism and of unreasonable opposition. But we have lived to witness, what I may term, a great reparation. Even those who did not know the man, and the qualities of mind and heart which endeared him to so many, have come to recognise that in his work he was actuated by a single-hearted desire to discover the truth; and, after calm reflection, they have conceded that his studies and his views like all studies and all views which are based upon the truth, not only are not irreconcilable with, but add to our conceptions of, the dignity and glory of God. And here I may be allowed to remark that it is impossible to study the writings of Darwin, and especially the one in which he treats of "The Descent of Man," without recognising an undercurrent of reverent sentiment, which in one or two places finds expression in words telling us that man differs from the animal creation, if not in physical characteristics which cannot be bridged over, at least in moral attributes and in the "ennobling belief in God," by his power of forming that conception of the Deity which, to use Darwin's own words, is, "the grand idea of God hating sin and loving righteousness." ("The Descent of Man and Selection in Relation to Sex." Second edition (1874), page 144.)

We cannot help mourning for our great ones, though they be taken from us in the fulness of years, and when their labours have been so numerous and so productive that we marvel that they have been able to achieve so much within the span of a single life; but our grief is immeasurably greater when the man of genius is taken from us in the plentitude of strength, as it were upon the threshold of a life full of extraordinary promise.

Francis Maitland Balfour, whose sudden death has so recently cast a gloom over us all, was a man who appeared destined to advance our knowledge of animal development more than it had been advanced by the labours of any one of his predecessors. His death recalls the train of thought which we have pursued when reflecting upon the lives and works of such men as Mayow and Bichat, Gerhardt and Clifford. If so much could be achieved in so short a life, what great benefits would science not have derived, what remarkable steps in advance might not have been made, had it been given to these great minds to work on for the good of their race during a lifetime of ordinary length. It must be sufficient for us that it was destined otherwise; and, in mourning for our departed friend, we may at least reflect that we would not have him less worthy of our admiration in order that we might mourn him the less.

The Researches of Francis Maitland Balfour.

At the risk of having to be somewhat brief in my discussion of the subject proper of this address, I must yield to the impulse which leads me to give you some account of Balfour's work.[1]

Having been educated at Harrow, Balfour entered Trinity College, Cambridge, in the year 1870. His friend and master, Michael Foster has told us how, from the very first, besides engaging in systematic studies which he was able to pursue with no small degree of success, he devoted himself with passion to original research. At the very outset Balfour engaged in work which led to speculations of a fundamental and far-seeking nature, and of the three embryological papers, (*Studies from the Cambridge Physiological Laboratory*. Part I., 1873. *Quarterly Journal of Microsopical Science*, vol. xiii., 1873.) which he wrote before taking his degree, two related to questions which occupied his attention in a special manner to the end. One of these, "On the Development and Growth of the Layers of the Blastoderm," contains several statements not afterwards maintained; for instance, as to the independent origin of the mesoblast in the chick, where it is said "neither to originate from the epiblast nor from the hypoblast, but to be formed coincidentally with the latter, out of apparently similar segmentation cells." The other, "On the Disappearance of the Primitive Groove in the Chick," calls attention to, and corroborates Dursy's discovery of seven years before, and closes with a suggestion of the great hypothesis (afterwards elaborated) that the primitive streak is a lingering remnant of the blastopore. Balfour also wrote whilst an undergraduate "On the Development of the Blood-vessels in the Chick," but it may be doubted whether he advanced our knowledge of this obscure subject.

The "Elements of Embryology," by Michael Foster and Balfour, appeared (1874) shortly after Balfour had taken his degree (1873), and Foster has generously recorded how great was the part his pupil took in the production of this book. The month after taking his degree he made his first journey to Naples, and it was whilst working there that he entered upon his remarkable investigation on the development of Elasmobranchs. The natural outcome of Gegenbauer's exposition (Gegenbauer, "Das Kopfskelett der Selachier," 1872) of the primitive character of this group was that increased interest should attach to all researches on its embryology. To an introductory account of the embryology of Elasmobranchs (*Quarterly Journal of Microscopical Science*, vol. xiv., 1874.) Balfour owed, I believe, his fellowship at Trinity College, and from that time onwards until 1878 he pursued the investigation at Naples and in Cambridge. The collected results appeared in 1878, as "A Monograph on the Development of Elasmobranch Fishes." No research upon a limited group ever contained more numerous or more wide generalisations, extending over the whole domain of vertebrate embryology. I may dwell for a few moments upon some of its most interesting sections.

The structures which we are now familiar with as "head-cavities" are described for the first time, and named; their relation to the cranial nerves and their resemblance or equivalence to the muscle plates of the body are pointed out; and Balfour seizes upon their value in throwing light upon the great problems of the segmentation of the head and the segmental value of the cranial nerves. In particular the 5th nerve and the 7th, with the auditory, are specified as the segmental nerves of the mandibular and hyoid segments. The short, but very important, notice of the sympathetic system showed that its ganglia developed on branches of the spinal nerve, and that it was therefore a product of the epiblast ("Elasmobranch Fishes," p. 172.) The primitive features of the mesoblast and notocord and their hypoblastic origin are described, ("Elasmobranch Fishes," pp. 49, 85, 92, 104.) and furnish material for the comparison afterwards instituted in the "Comparative Embryology" (vol. ii., pp. 243, 246.) between their development in Elasmobranchs and their still more primitive origin in Amphioxus, as diverticula of the archenteron. A very able chapter on excretory organs concludes this monograph. This subject had engaged Balfour's attention very early, and his introductory account of Elasmobranch Development contains his discovery of segmental organs in Elasmobranchs,—a discovery made independently but simultaneously by Professor Semper. These organs are shown to develop in the mesoblast, and are compared with the segmental organs of annelids.

A paper published in 1876 gives a singularly clear and thorough *résumé* of our knowledge of the development of the urino-genital system; and the diagrams there given, illustrating the homologies of the male and female urino-genital organs, are wonderfully simple and instructive. Shortly after the publication of this paper, Balfour became a Fellow of the Royal Society, for which he received a Royal Medal in 1881.

Among the interesting points that Balfour had made clear in connection with the spinal nerves of Elasmobranchs, was the fact that the anterior and posterior roots arise alternately, and not in the same vertical plane. He sought for an explanation of this in Amphioxus at Naples, in 1876. Owsjannikow and Stieda had discovered that the nerves of the opposite sides in Amphioxus arise alternately, and Stieda further stated that the nerves of the *same* side arise alternately from the dorsal and ventral corners of the cord. Stieda considered that two adjacent nerves were together equivalent to a single spinal nerve of higher vertebrates. Balfour (*Journal of Anatomy and Physiology*, vol. x., 1876.) found no trace of difference of level in the origin of nerves on the same side, *i.e.* he denied the existence of ventral or anterior roots; and afterwards, in investigating the cranial nerves of higher vertebrates, and being unable to find any trace of anterior roots, he framed the bold hypothesis ("Elasmobranch Fishes," p. 193, "Comparative Embryology," vol. ii., p. 380.) that the head and trunk had been differentiated from each other at a time when mixed motor and sensory posterior roots were the only roots present, and that cranial and spinal nerves had been independently evolved from a common ground-plan.

Balfour's investigation of the development of the ovary was incomplete when his work on Elasmobranchs appeared; and he continued to work at this subject, both in Elasmobranchs and Mammals, publishing upon it in 1878 (*Quarterly Journal of Microscopical Science*, vol. xviii., 1878.) A paper published in the same year, on the "Maturation and Impregnation of the Ovum," contained the very ingenious suggestion that the casting out of the polar bodies prevents the ovum developing by itself into a new individual, *i.e.* prevents parthenogenesis; and Balfour points out that parthenogenesis is practically confined to the arthropoda and rotifera, which are the only two groups in which polar bodies are not known to occur.

Balfour still continued, now in conjunction with Sedgwick, his researches on the urino-genital system, and described, among many other new points, the existence of a head-kidney (pronephros) in the chick (*Proceedings* of the Royal Society, vol. xxvii., 1878).

In this year, Balfour also investigated (*Quarterly Journal of Microscopical Science*, vol. xix., 1879.) the early development of Lacerta, and pointed out the presence of a primitive streak and of a neurenteric canal. This investigation confirmed his belief in the hypothesis previously quoted that the primitive streak is the relic of a blastopore.

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[1] In the preparation of this part of my address I have been very greatly aided by one of Balfour's pupils, my nephew, D'Arcy W. Thompson, Scholar of Trinity College.

At this time Balfour was working hard at his text-book of "Comparative Embryology." His published papers were no less numerous than before, but consisted in part of extracts from the more speculative chapters of the forthcoming book. He, however, published a paper, (*Quarterly Journal of Microscopical Science*, vol. xx., 1880.) containing the results of work scattered over two years, on the development of Spiders. He also published a paper (*Proceedings* of the Zoological Society, 1881.) on the skeleton of the paired fins, based upon his work on Elasmobranchs. In this he contests the views of Gegenbauer and Huxley, that the primitive fin consists of a central multi-segmented axis with many lateral rays, and is most nearly retained in Ceratodus; he rather considers the primitive form to be a longitudinal bar running along the base of the fin (basipterygium), and giving off at right angles a series of rays which pass into the fin. He adheres to the view expressed in the "Elasmobranch Fishes," (p. 101.) that the vertebrate limbs are remnants of two continuous lateral fins.

Another important paper of the same year dealt with the placenta. Balfour supposed that in the primitive Placentalia, simple fœtal villi, like those of the pig, projected from the discoidal allantoic region of the chorion into uterine crypts. The deciduate discoidal placenta of Rodents and Insectivores is the first stage in advance of this primitive type. Then along different lines diverge the zonary placenta of Carnivora, and the diffuse placenta of Suidæ, Lemuridæ, &c.; and the latter becomes contracted down to the discoidal placenta of man, a form in no way to be confounded with the primitive discoidal placenta of Rodents.

He engaged also, in conjunction with Mr. W. N. Parker, in a very important research; to be published in full in the "Philosophical Transactions," on the "Structure and Development of Lepidosteus." This paper contains an immense amount of new matter, both anatomically and embryological, and shows that Lepidosteus, though a true ganoid, has very marked teleostean affinities.

Balfour's last published paper, (*Quarterly Journal of Microscopical Science*, vol. xxii., 1882.) which appeared during his recent illness, was written with the assistance of Mr. Deighton, and related to the germinal layers of the chick. This paper describes, in a very beautiful way, the double origin of the mesoblast, partly from an axial strip of epiblast in the line of the primitive streak, and partly as two lateral plates differentiated from the hypoblast in front of the primitive streak.

Before his last, fatal journey, Mr. Balfour was engaged in preparing a new edition of the "Elements of Embryology," and in producing a very elaborate memoir on the "Anatomy and Development of Peripatus." He had previously investigated that animal, in 1879, and had cleared up the matter of its segemental organs (overlooked by Moseley), and demonstrated the presence of ganglia on its ventral nerve-cords.

Mr. Balfour became a member of this Association in 1871, the year after he entered Trinity College. At the brilliant Belfast meeting in 1874 he read his first paper before the Association on Elasmobranch Fishes; and this paper and Balfour's share in the keen discussion which followed are still remembered with admiration by many. In 1880, at Swansea, he delivered an address, as Chairman of the sub-section of Anatomy and Physiology, dealing with the mutual services rendered by the evolution theory to embryology, and by embryology to the evolution theory, with special reference to the developmental history of the nervous system. In 1881, he was appointed one of the two General Secretaries.

But the great text-book of comparative embryology ("Comparative Embryology," vol. i., 1880, vol. ii. 1881) is the real monument of Balfour's fame. It is impossible to convey an idea of the merits of this book. It grappled with the enormous mass of scattered literature upon the subject, and formed it all into a consecutive account, clear and accurate. Discordant statements were weighed and estimated, frequently brought into harmony by an ingenious explanation or by a new and crucial observation. Countless investigations were repeated and verified, and countless suggestions of important work, that still remains to be done, make the book as valuable to the *savant* as to the student. Among the chapters ("Comparative Embryology," vol. ii., chap. xi. xii. xiii.) most remarkable for broad and philosophic generalisations are those dealing with the "Ancestral Form of the Chordata," "Larval Forms," and the "Origin and Homologies of the Germinal Layers." Balfour accepts the gastrula as a stage in the evolution of the metazoa, and leans somewhat to invagination, as the more primitive process than delamination in the production of the gastrula. He shows distinctly that the mesoblast arose in the first instance, not independently, but as a differentation from the other two layers, and that the mesoblast is a homologous structure throughout the triploblastic metazoa. In the chapter on "Larval Forms" he gives numerous reasons and arguments for a larval development repeating the ancestral history, better and more fully than a fœtal development; he reviews the types of larvæ (discriminating six types), the cases tending to produce secondary changes in larvæ, and suggests, as a hypothesis for the passage from the radial to the bilateral type, that in a pilidium-ilke larva the oral face elongated unequally, an anterior part forming a præ-oral lobe, and a posterior outgrowth the trunk, while the aboral surface became the dorsal surface. He suggests that adult Echinodermata have retained, and not secondarily acquired, their radial symmetry, and considers a radially symmetrical organism, like a medusa, as the prototype of all the larval forms above the cœlenterates. Balfour does not admit the specially close relationship of the Chordata with the Chætopods, which Dohrn and Semper maintain; but considers that the Chordata descended from a stock of segmented worms derived from the same unsegmented types as the Chætopods, but in which two lateral nerve-cords like those of the nemertines coalesced dorsally instead of ventrally. He considers that the mouth in ancestral Chordata was suctorial, and was not formed, as Dohrn supposes, by the coalescence of two visceral clefts. Finally, Balfour draws up a scheme of the phylogeny of the Chordata, according to which the hypothetical protochordata, with a notochord with a suctorial mouth and very numerous gill-slits, acquired one by one, vertebræ, jaws, an air-bladder, a pentadactyl limb, an amnion: each new accession characterising a hypothetical protogroup, from which some existing group is supposed to have diverged.

Those of my hearers who had not followed Balfour, scientific labours, but who merely knew him as one of the most respected workers in the field of biology, will I trust, even from my brief sketch, have formed some idea of the activity and originality of his mind, and will understand how his death has occasioned a feeling almost akin to despair, in that he occupied a place which it appears to us now impossible to fill. "How are the mighty fallen, and the weapons of war perished!"

ON THE GROWTH OF OUR KNOWLEDGE OF THE PROCESS OF SECRETION IN THE ANIMAL KINGDOM.

The Views of the Ancients.—It was known to the ancients that organs of the body exist which are concerned in the separation from it of excrementitious substances, although the greatest doubts prevailed as to the organs to which such functions should be ascribed. Thus we find Hippocrates defining it as characteristic of glands that they occur in moist parts of the body; but showing his ignorance of the true relations of glands to secretion by connecting them with the formation of hairs, and discussing the question which we find our own Wharton debating again in the seventeenth century, and which he formulates, "Num cerebrum ad glandularum numerum vel viscerum accedat." The general opinion of the ancients, and the opinion which was adopted and by Galen, was that the glands were sieves or collanders (cola), which served to strain off from the blood purely excrementitious substances. The liver and kidneys were strangely enough removed from the group of glands and placed amongst the viscera. The first writer who appears systematically to have treated of the glands was the before-mentioned Wharton in his "Adenographia sive glandularum totius corporis descriptio." Although this author certainly added to the existing knowledge of the descriptive anatony of secreting organs, his views on the functions of glands were strangely fanciful and erroneous.

The glands he considered to be especially related to the nervous system, the viscera, so-called, to the blood-vessels; such glands as the pancreas, and the salivary and lachrymal glands being engaged in separating excrementitious substances from the nervous system. It was in 1665 that the great anatomist Malpighi ("Exercitatio Anatomica de Renibus") first attempted to investigate the structure of glands in a truly scientific spirit, endeavouring to establish a relationship between simple glandular follicles and such complex glands as the liver. All glands he believed to contain as ultimate elements bodies which he termed "*acini*," a word which in its primitive classical sense has been used to designate the stone or seed of the grape or the grape itself. The conception, indeed, which Malpighi formed of an "*acinus*" was rather that of a secreting nodule than of an ultimate saccular or tubular recess. The "*acini*," however, he believed to be in communication with the efferent ducts of the glands to which they belonged, and through which they poured out their proper secretion, derived in the first instance from the blood contained in minute arteries supplied to the gland. Ruysch (1696), known as the first celebrated injector of blood-vessels, finding that frequently the fluids which he forced into the blood-vessels of glands escaped through their ducts, or made their way into the surrounding tissues, concluded that the blood-vessels communicated directly with the interior of the glands; these he held to be organs which, according to the views that had long prevailed, merely strained off from the blood certain of its more liquid constituents. The views entertained by the most eminent of the supporters of Ruysch, the illustrious Haller were expressed by him as follows. After defining the term "acinus" to signify the ultimate division of a gland, he remarks that "the acini consists of congeries of vessels, bound firmly together with the cellular web, containing an excretory duct in their interior, which commences from the most minute arteries by small ducts impervious to the blood. . . . So that secretion differs from the ordinary circulations of the blood in this particular, that the smallest arteries are continuous with veins of equal or greater size, capable therefore of receiving the blood, whilst the excretory ducts are much smaller, in order to effect the separation of the secretion." (Haller, p. 275.) The advocates of the Ruyschian theory were compelled to have recourse to the most improbable hypotheses to explain the diversity of the secretions of different glands, as, for example, that different glands secrete different liquids, because of the difference in the diameters of the pores by which the blood-vessels communicate with the glands; that the different arrangement of blood-vessels, the mode in which they divide, the resistance which they offer to the flow of blood through them, by modifying the pressure and velocity of the blood-flow through the organ, induce secretions varying in character. It is strange to learn from Haller, as was indubitably the fact, that the great majority of his contemporaries, such men as Peyer and Vieussens, and even Boerhaave, adopted the Ruyschian view of the structure of glands. The opposition to Ruysch came first from Ferrein (Ferrein, "Sur la Structure des Glands," &c., *Mémoires de l'Acad. Roy. des Sciences de Paris*, 1749), who maintained that the kidneys essentially consist of an assemblage of convoluted tubes, which he looked upon to be the seat of the renal secretion—tubes which a subsequent investigator, Schumlansky (Schumlansky, "Dissertatio Inaugur. Anatomica de Renum structurâ, A gentoreti, 1880), looked udon as taking their origin in the *acini* of Malpighi, to which he referred the active part in secretion. Then followed the researches of Mascagni and Cruickshank, who found, by injecting quicksilver into the mammary glands, that the ramification of the ducts of this organ terminate in racemose follicles; though Mascagni still admitted a connection, by means of open pores, between the sides of the glandular blood-vessels and the interior of the glands themselves. It was unquestionably Professor E. H. Weber, of Leipzig, who completely demolished the Ruschyian hypothesis, and who by numerous researches on the salivary glands of birds and of mammals, and on the pancreas of birds, established the general fact of the termination of gland ducts in blind extremities, though with modesty he put forward his opinions as confirming the inductions of Malpighi, expressing himself as follows: "Admirably did Malpighi avail himself of the structure of the liver in the lower animals, and to the embryo of the higher, is a foundation-stone for his opinions; for the arrangement of the whole glandular system speaks for itself, inasmuch as it simply consists of single, compact, hollow, blind canals, more or less numerous, floating in the fluid which surrounds their organs; and, although these ramifications are drawn out between the branches of the blood-vessels, there is no immediate passage from one to the other."

THE RESEARCHES OF JOHANNES MÜLLER.

Such was the state of knowledge in reference to the structure of secreting glands and secretion at the time when the great Johannes Müller undertook the investigation of which the results were first of all published in the memorable work entitled "De Glandularum secernentium Structurâ penitiori earumque prima Formatione" (Lips. 1830). It is impossible not to sympathise with the reflection of Professor Heidenhain, recently made in reviewing the researches of Johannes Müller in connection with this subject (Heidendain in Hermann's "Handbuch der Physiologie," vol. v., 1880, p. 6.), to wit, that the physiologists of to-day may be accused of ingratitude for having allowed the great name of Johannes Müller to have well-nigh disappeared from the pages of physiological literature. We forget that this man—this giant in the field of biology as he is appropriately termed by Heidenhain, the last man of whom perhaps it will ever be said that he was at once the greatest comparative anatomist of his time and the most philosophical and original of all contemporary physiological writers—by his own researches, and particularly by the one which concerns us to-day, influenced the progress of physiology, at a most critical period, more than any other man. He was not, like his contemporaries Magendie and Flourens, a great physiological experimenter, though he showed that he well appreciated the value of experiment in advancing our science; but he was pre-eminently a physiologist who recognised the immense importance of a close study of structure, not only because of the interest which it presents to the pure and philosophical morphologist, but because of its absolute necessity, if we are to penetrate at all deeply into the secrets of animal function. Müller, in the first instance, had convinced himself, by the study of the circulation of organs sufficiently transparent to permit of it, especially the circulation through the liver of larval salamanders, that, in glands, arteries never end in any other mode than by capillaries leading into veins. He then set himself to study in the case of most glands, and in a large variety of animals, the relationship of gland ducts to the truly secreting parts of the organ, and the relation of the blood-vessels to these. Basing himself upon these anatomical studies of adult organs, and upon a careful study of the development of glands—a study which had been attempted slightly by Malpighi, and more satisfactorily in the case of the parotid by E. H. Weber (E. H. Weber, "Beobachtungen über die Structur einiger conglomerirten und einfachen Drüsen und ihre erste Entwickelung." Mickel's *Archiv* for 1827, p. 274)—Müller came to the conclusion that all glands possessed of a duct are only involutions more or less complex of membranes, the largest number being involutions of the external investment of the body or of the membranes opening upon its surface. The following are the general results relative to the structure of glands which Müller deduced from the anatomical study of individual organs:—[1]

1. However various the forms of their elementary parts, all secreting glands without exception (not only those of the human body, but all met with in the animal kingdom) follow the same law of conformation, and constitute an uninterrupted series from the simplest follicle to the most complex gland.

2. No line of demarcation can be drawn between the secreting organs of invertebrata and those of vertebrate animals; not merely do we meet with the simplest sacs and tubular secreting organs, like those of insects, in the higher animals, but there is a gradual transition from these simple secreting organs to the glands of the most perfect vertebrata.

3. All glands agree in affording by their interior a large surface for secretion. The varieties of internal surface by which the great end—extent of surface in a small space—is attained, are very numerous.

4. *Acini*, in the hypothetical sense in which the term has been used by writers—in the sense viz. of secreting granules—do not really exist; there are no glomeruli of blood-vessels with ducts arising from them in a mysterious way, as has been supposed, whatever notions may have been held regarding them.

5. The parts described as *acini* are merely masses formed by the agglomeration of the extremities of the secreting canals; frequently, indeed, they are formed of minute vesicles aggregated together in grape-like bunches, which may be injected with mercury, and are often susceptible of inflation.

6. In many glands which have been incorrectly described to have *acini* or secreting granules, there are not even the hollow vesicular *acini*; the secreting tubes, instead of terminating in vesicles or cells, form long convoluted canals or straight tubuli or short cæca.

7. It has been demonstrated in the case of all glands that the blood-vessels are not continuous with the secreting tubes—that the minute vessels bear the same relation to the coats of the hollow secreting canals, and their closed extremities, as to any

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[1] This abstract of Müller's general conclusions has been abbreviated from the sections treating on this subject in his "Elements of Physiology." See Translation by Dr. Baly, London, 1138, vol. i. p. 456, *et seq.*

other delicate secreting membrane, such as, for example, the mucous membrane of pulmonary air cells.

8. The arborescent ramification of the blood-vessels accompany the ducts in their development, and the reticulated capillaries in which the blood-vessels terminate are extended over all the closed elementary parts of the gland and supply them with blood. In the chick we may observe the simultaneous development of the two systems; in proportion as the development of internal surface from a plain membrane to cæcum and ramified cæca proceeds, the vascular layer of the originally simple membrane is raised on the exterior of the efflorescence.

9. The ramified canals and tubes, which when the structure is simple, as in insects and crustacea, and even in some glands of the mammalia, lie free and unconnected, become more aggregated together, and acquire a common covering, in proportion as their evolution is carried further; and thus is produced a parenchyma or solid organ.

10. The capillary blood-vessels are for the most part much more minute than the smallest branches of the ducts of secreting canals and their cæcal extremities, even in the most complex glandular organs. The elementary parts, of glands, though minute, are of such a size that the capillary blood-vessels form around them a network which invests them.

11. The formation of the glands in the embryo displays the same progressive evolution from the simple to the complex state as is observed in ascending the animal scale. The most perfect and complex glands of the higher animals, when they first appear in the embryo of these animals, consist merely of the free efferent ducts without any branches, and in that state exactly resemble the secreting organs of the lower animals. The glands are formed from the unbranched tubes by a kind of efflorescence or ramification.

12. The mode in which the extent of internal secreting surface of a gland is realised is very various; and no one kind of conformation is peculiar to any kind-of gland. Perfectly different glands may have a similar elementary structure, as is the case, for instance, with the testes and the cortical substance of the kidneys. And similar glands have often a perfectly different structure in different animals; of which the lachrymal glands, examined in the chelonia, birds, and mammalia, afford an example.

Johannes Müller, recognised thoroughly, as we have seen, that the character of a secretion cannot be deduced from the structure of the organ which produces it. Was he able to throw any light upon the mystery which had baffled his predecessors and to explain the cause of the specific endowments of the different glandular organs? Let us allow Müller to speak:—

"The peculiarity of secretions does not depend on the internal conformation of the glands; for, as I have sufficiently demonstrated, each secretion is in different animals the product of the most various glandular structures, and very different fluids are secreted by glands of similar organisation. The nature of the secretion depends therefore solely on the peculiar vital properties of the organic substance which forms the secreting canals, and which may remain the same, however different the conformation of the secreting cavities may be; while it may vary extremely although the form of the canal or ducts remain the same." It was the living lining substance of the gland which, according to Johannes Müller, formed the secretion, at the expense of materials which it obtained from the blood of contiguous capillaries. This living substance lining the inner recesses of the glands had not yet been differentiated into its constituent units, the secreting cells, and therefore Müller's statement wanted a certain definiteness, though, so far as he went, he was perfectly accurate.

THE RESEARCHES OF JOHN GOODSIR.

The success with which that eminent pupil of Johannes Müller, Theodore Schwann, had extended the generalisations of Schleiden (on the part taken by the cell in the formation of vegetable structures) to the elucidation of the animal tissues, had given the greatest impulse to the study of animal histology, and a large number of observers, especially in Germany and England, were directing their attention to the discovery and study, in all tissues and organs, of the all-important cells.

Purkinje had announced the hypothesis that the nucleated epithelium which he discovered to line the gland ducts might exercise secreting functions. Henle had described with great minuteness the epithelium cells which line the ducts of the principal glands and follicles, and which form the most superficial structures of mucous membrane, and Schwann had suggested that this epithelium probably played a part in the act of secretion. It was, however, unquestionably the Scottish anatomist, John Goodsir, to whom was reserved the merit of establishing in an indisputable manner the fact that the essential and ultimate secreting structures in glands are the morphological units, the gland cells. As Johannes Müller had examined the arrangements and coarser structure of glands throughout the animal kingdom, with the result of discovering the general plan of gland-structure, and the analogies existing between glands, however diverse, so John Goodsir passed under review the histological characters of the cells of different glands in a large variety of animals, vertebrate and invertebrate. His first results were published in the "Transactions of the Royal Society of Edinburgh" for the year 1842; his more matured views were developed in a paper entitled "On Secreting Structures," which formed one of a collection of papers which saw the light in 1845. As a result of his survey Goodsir came to conclusions of which the most important may be stated, almost in his own words, as follows:—

"The ultimate secreting structure is the primitive cell endowed with a peculiar organic agency, according to the secretion it is destined to produce. I shall henceforward name it the primary secreting cell.

"Each primary secreting cell is endowed with its own peculiar property, according to the organ in which it is situated. In the liver it secretes bile, in the mamma milk, &c.

"The primary secreting cells of some glands have merely to separate, from the nutritive medium, a greater or less number of matters already existing in it. Other primary secreting cells are endowed with the more exalted property of elaborating, from the nutritive medium, matters which do not exist in it.

"The discovery of the secreting agency of the primitive cell does not remove the principal mystery in which the function has always been involved. One cell secretes bile, another milk; yet the one cell does not differ more in structure from the other than the lining membrane of the duct of one gland from the lining membrane of the duct of another. The general fact, however, that the primitive cell is the ultimate secreting structure, is of great value in physiological science, inasmuch as it connects secretion with growth, as phenomena regulated by the same laws."

Goodsir was unquestionably wrong in certain of his speculations concerning secreting cells; as, for instance, in attributing at one time the chief part in the process of secretion to the cell wall, at a later period ascribing the same function to the cell nucleus. He certainly had not grasped the modern idea, which, as I shall afterwards more particularly point out, considers the act of secretion as one of the results of the activity of the living protoplasm of the cell. His assumption, too, that the secreting cell invariably contains, preformed, the characteristic matters of the secretion, is one which is by no means generally true. Nevertheless, it is impossible to study Goodsir's researches on the secreting cell, without ascribing to him the merit of having been the one who made the most important generalisation, connecting cell life with a definite organic function.

I may be permitted, as it were parenthetically, to refer for a moment to John Goodsir, with the veneration which is natural in one who was his pupil. If it be true that the rapid march of scientific discovery has caused us well-nigh to forget the great debts which we owe to Johannes Müller, it is no less true that John Goodsir's name has passed into premature and undeserved oblivion. Goodsir's was a mind which in some respects, especially in its tastes, resembled that of Müller. He was a devoted anatomist, and studied morphology in the first instance for its own sake, but also because of the light which it sheds on organic function. He had a powerful intellect, an insatiable thirst for knowledge, a sympathy with all branches of inquiry which could throw light upon the science to which he devoted his life, and a devout and reverential spirit, which was not the less strong because it only rarely found audible, though then it was emphatic, utterance. In the earlier part of his scientific career, numerous papers, for the most part short, but characterised by remarkable originality of observation and freshness of thought, seemed to promise that Goodsir would be one of the most productive of the workers of his time. A lingering illness which, without altogether disabling him, enfeebled his physical powers, and cast a gloom upon a life which had promised so much, almost put an end to his career, in so far as the scientific world at large was concerned, and henceforward he devoted his remaining energies to studies of which the results were for the most part not published, but especially to the task of teaching. Goodsir was a master who, if judged of by the low standard of fitness to instruct the great majority of his pupils in such a manner as to enable them successfully to pass examinations, would occupy no exalted position. He possessed, however, the far rarer power of instilling into the minds of the best of his pupils that love of original inquiry, and that deep regard for truth which are the chief incentives to all scientific research of any real value.

THE INVESTIGATIONS AND THEORIES OF BOWMAN.

At the time when Goodsir was engaged in his investigations and speculations relating to cells, Mr. Bowman was making researches which were to give him a lasting place among the great histologists of the century.

His investigations on the structure of the kidney,[1] which was published in the "Philosophical Transactions" for the year 1842, surpassed in completeness as an anatomical study, no less than by the deep insight into the nature of the function discharged by the organ, any investigation of like kind which had preceded it. It not only led to a more complete knowledge of the structure of the kidney than was possessed of that of any other gland, but to far-seeing generalisations concerning the structure of mucous membranes, and of secreting organs generally, which found expression in a masterly article on mucous membranes, published in the year 1847, in the "Cyclopædia of Anatomy and Physiology."

Time will not permit of my giving a complete analysis of the (to use a German expression) epoch-making research upon the kidney; but let me remind you that it led to a complete understanding of the relations of the Malpighian bodies to the urinary tubules; to a description which, so far as it went, was perfectly accurate of the tubules themselves, though the scheme upon which these tubes are arranged has, since Bowman's time, thanks to the labours of Henle, Ludwig, and Schweigger-Seidel, been proved to be more complicated than he had imagined, and to a knowledge of the distribution of blood-vessels, not only in the kidney of man and other mammalia, but also in that of certain reptiles.

His study of the structure of the tubuli uriniferi had led Mr. Bowman to discover that in these, a layer of epithelial cells lies upon a structureless membrane, to which he gave the name of the *basement membrane*,[2] and which intervenes between the epithelium and the blood capillaries, whence the materials of secretion are primarily derived. His examination of the mucous membranes of the body led Bowman to the conclusion that the relationship so easily observed in the case of the kidney between cells, basement membrane, and blood-vessels, is one which holds true, not only in the case of that organ but in that of many other epitheliated structures.

"In the mucous tissue," said Mr. Bowman (Article, *Mucous membrane*, in Todd's "Cyclopædia," p. 436), "there are two structures which require to be separately described, viz., *the basement membrane* and *the epithelium*. The basement membrane is a simple homogenous expansion, transparent, colourless, and of extreme tenuity, situated on its parenchymal surface and giving it shape and strength. This serves as a foundation on which the epithelium rests. The epithelium is a pavement composed of nucleated particles adhering together, and of various size, form, and number. The following general observations on these elementary parts will receive illustration as we advance. Neither the one nor the other is peculiar to the mucous tissue in the sense either of being invariably present in it, or of not being found elsewhere. There are certain situations of the mucous system where no basement membrane can be detected, and others from which the epithelium is absent. Both, however, are never absent together. Again, a structure apparently identical with the basement membrane is met with in numerous textures besides the mucous, and all internal cavities, whether serous, synovial, or vascular, or of anomalous kind (as those of the thymus and thyroid body), are lined by an epithelium."

As a result of his anatomical studies on the kidney, Mr. Bowman was led to frame a theory of renal secretion, which, though opposed for a time by a master mind, has, by the progress of research, received complete confirmation, and which was based in no small degree upon the new views of the function of epithelial cells in glands. The Malpighian body, Bowman showed, is the dilated commencement of a convoluted tubule, and, like it, presents a delicate, structureless, basement membrane. Into the Malpighian body projects a tuft of capillary vessels, continuous, on the one hand, with an afferent vessel derived from a branch of the renal artery, on the other, with an efferent vessel of smaller size than the afferent; both afferent and efferent vessels piercing the capsule of the Malpighian body; after leaving the glomerulus, the efferent vessel breaks up into a series of capillaries, which are distributed to the walls of the convoluted tubes. The tuft of blood-vessels projecting into the Malpighian body, Bowman described as being perfectly bare, that is to say, not covered by a basement membrane, or by a layer of epithelium cells. This part of his description has not been confirmed by recent work, the more delicate methods of modern histology allowing of a ready demonstration of a layer of cells of extreme tenuity covering the glomerulus.

The basement membrane of the convoluted tube was described as lined by a nucleated epithelium of a finely granular opaque aspect; the neck of the tube, where it joins the Malpighian capsule, and the contiguous portions of the capsule were described as covered by a layer of cells, differing altogether from the first, being much more transparent, and possessing in certain animals vibratile cilia. In some cases the whole interior of the capsule was lined by epithelium cells of great delicacy and tenuity; in others, these cells could not be traced over more than a third of the capsule. Basing himself upon the altogether exceptional arrangement of the blood-vessels of the glomerulus, Bowman advanced the theory that this is a structure destined to separate from the blood its watery portion. The epithelium of the convoluted tubes on the other hand, which Bowman pointed out to be eminently allied to the best marked examples of glandular epithelium," he believed to be concerned in the separation of the characteristic solid matters of the renal secretion.

I shall for the present conclude my remarks upon Mr. Bowman's investigations and theoretical views by stating that, by his investigations of the blood-supply to the kidney of the boa constrictor, he gave the strongest proofs which could be derived from anatomical evidence of the correctness of his views, and furnished great part of the knowledge required for the subsequent researches which Nussbaum made on the secretion of the newt's kidney, and which afforded the most conclusive experimental evidence in favour of the theory which Bowman had advanced.

THE DISCOVERIES OF CARL LUDWIG.

If to Johannes Müller we must ascribe the greatest share of merit as a discoverer of the general affinities, relationships, and functions of glands, it appears unquestionable that to Carl Ludwig belongs the credit of having, above all others, brought the light of experimental physiology to bear upon the subject of secretion.

Ludwig is one of the most eminent of the physiologists who have endeavoured, as far as possible, to apply the conceptions derived from a study of physical and chemical processes in general, to the elucidations of the functions of the organism. More than anyone else has he successfully adapted the methods of research of the chemist and of the physicist to the investigation of the problems which lay before him. Above all others he is to be spoken of as the great teacher amongst all of the great teachers of physiology which this century has produced. If we try to find one who, from the fertility of his mind and the influence which he had upon men of ability, affected the progress of his science in like measure to Ludwig, we revert to the name of Liebig. When I say that physiology owes as much to Ludwig as chemistry to Liebig, I shall, I feel sure, be doing but scant justice to the great man, who at Marburg, at Vienna, and at Leipzig, has won for himself the right to be called at once the greatest physiologist, and the greatest teacher of physiology, of his time.

1. *Ludwig's Discovery of Secreting Nerves.*—It was in the year 1851 that Ludwig first announced to the scientific world (Ludwig, "Neue Versuche über die Beihilfe der Nerven zur Speichelabsonderung," Henle & Pfeifer's *Zeitschrift*, New Ser., vol. i. (1851), p. 255) the fact that the secretion of the salivary glands is under the influence of the nervous system. C. G. Mitscherlich, as Ludwig points out, had surmised that the secretion of saliva only occurs as the result of a stimulation of certain nerves. *i.e.*, the nerves of taste and the nerves supplying the muscles of mastication. No attempt had, however, been made, before Ludwig's, to ascertain experimentally whether the stimulation of nerves supplying glands influenced directly their secretion. As a subject of study Ludwig chose the submaxillary gland. He found that on stimulating by a succession of induc-

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[1] W. Bowman, "On the Structure and Use of the Malpighian Bodies of the Kidney, with Observations on the Circulation through the Gland," *Philosophical Transactions* for the year 1842, Part I., p. 57.

[2] *Op. cit.*, p. 58.

tion shocks the nerve twigs proceeding from the lingual branch of the fifth nerve, and which accompany Wharton's duct to the gland, secretion of saliva occurred, so long as the excitability of the nerves persisted.

In experiments performed in conjunction with his pupil Rahn, Ludwig found that secretion occurs on direct stimulation of the glandular nerves, even when the circulation has been arrested for a time, as for instance, when the contractions of the heart are exhibited for some time.

2. *Ludwig's Discovery that Secretion is not a Process directly dependent upon the Arterial Pressure.*—In the paper which I have already quoted, Ludwig published the results of the following experiments. A mercurial gauge was placed in communication with the duct of the submaxillary gland, the height of the mercury in the guage being recorded (by means of a float to which was attached a writing point) upon the travelling surface of the kymographion, the instrument which Ludwig had contrived for permanently recording the amount and variations of the blood pressure in arteries and veins. At the same time, another gauge placed in communication with the carotid artery, or one of its branches in close proximity to the gland, recorded the height of the blood pressure on the same travelling surface. On stimulating the secretory nerves, Ludwig found that saliva was poured out long after the pressure exerted by it upon the interior of the gland (as measured by the height to which the mercury was raised in the gland-duct manometer) exceeded the pressure of blood in the arteries. Thus in his first recorded experiment the mean pressure of blood in the carotid artery amounted to 108·5 millimetres of mercury, whilst during a stimulation of the nerve filaments going to the gland, the pressure in the gland-duct manometer rose to between 190·7 and 196·5 millimetres, *i.e.*, indicated that the pressure exerted by the fluid, secreted under the influence of nerve stimulation, exceeded the arterial pressure by an amount corresponding to a column of mercury about 3½ inches high. It is obvious that the experiment at once and conclusively proved that the secretion of a watery liquid like the saliva may be brought about by a process altogether different from a process of filtration; for in filtration the passage of liquid through the minute pores of the filter necessarily depends upon a difference in pressure on the two sides of the filter, the movement of liquid being from the side of greater to that of lesser pressure.

In this brief sketch I have only time to refer to the most salient of the early discoveries of Ludwig on secretion, and must pass over without comment the first experiments by which he showed the influence exerted by variations in the strength of the stimulus of secretory nerve upon the amount and chemical composition of the secreted liquid.

3. *Ludwig's Discovery that during Secretion Heat is evolved in Glands.*—Pursuing his researches on the salivary glands, Ludwig some years later, (Ludwig u. Spiess, "Sitzungsber. d. Wiener Akad. Mathem. u. Naturwissenchaft: Classe," vol. xxv. (1857), p. 548,) in conjunction with his pupil Spiessi discovered that, when a gland is thrown into action by stimulation of its nerves, heat is evolved. In the case of the submaxillary gland, for instance, he found that the saliva which was secreted might have a temperature nearly three degrees Fahr. (1·5° C.) above that of the blood going to the gland. Important as was this result because of the light which it threw upon the source of animal heat, its value as bearing upon the nature of the process of secretion was even greater. From the fact that the saliva is a liquid containing but three or four or five parts of solid matters to one thousand of water, it would scarcely have been surmised, upon a merely physical hypothesis, that its production would have been attended by any considerable evolution of heat. The evolution of heat is indeed one of the strongest proofs we have that the act of secretion is the result of the living activity of those ultimate units of the glands, the gland cells; but to this I shall revert hereafter.

The Researches of Schiff, Eckhardt, and Claude Bernard, on the Secretory Nerves of the Salivary glands.

The study of the innervation of the salivary glands which had been commenced by Ludwig and Rahn was continued with great success by other observers, and particularly by Claude Bernard and Eckhardt. The first of these observers proved the correctness of Schiff's supposition that the abundant secretion which followed the stimulation of fibres of the fifth cranial nerve was in reality due to the presence of fibres of the chorda tympani mixed with them. It was Eckhardt, however, and afterwards Claude Bernard, who established the remarkable fact that, in the case of the submaxillary gland, and, as has since been shown, of some other glands also, the gland is under the direct control of two orders of nerve fibres. The first are contained in branches of cranial nerves, and in the case of the submaxillary gland are derived from the facial nerve, and, when stimulated, lead to an abundant secretion of watery saliva, relatively rich in saline and poor in organic constituents; the second are contained in the so-called sympathetic nerve trunks distributed to the gland; and these, when stimulated, occasion an exceedingly scanty flow of very concentrated and highly viscid saliva, containing a relatively large quantity of organic constituents, particularly of mucin.

Claude Bernard now pointed out that stimulation of the above-mentioned nerves leads to changes in the circulation of blood through the gland, in addition to the changes in the amount and quality of the fluid secreted by it.

Thus stimulation of the cerebral fibres supplying the chorda tympani was found to produce a great dilatation of the arteries of the gland; so that the amount of blood passing through it was very largely increased, that passing out through the venous trunks of the gland presenting a florid arterial colour instead of the brown venous hue observed when the gland was not secreting. Stimulation of the sympathetic fibres, on the other hand, caused a great contraction of the glandular arteries, consequently a diminution of the flow of blood through the gland and into the veins, the blood presenting under these circumstances an intensely venous hue.

The facts just referred to appeared reconcilable at first with the view that the secretion of saliva, as a result of nerve stimulation, was primarily dependent upon changes in the circulation of blood through the gland; though, upon reflection, the surmise was negatived by some of the facts discovered long before by Ludwig, and particularly by that, already referred to, of glandular secretion following stimulation of glandular nerves, even where the circulation has been stopped, as by cardiac inhibition.

Bernard's experiments had unquestionably established that in addition to nerves which, when stimulated, occasioned the contraction of arteries—"the vaso-motor" or, as we now sometimes call them, the "vaso-constrictor" nerves—there are others which when stimulated occasion, on the contrary, the dilatation of arteries—the so-called "vaso-inhibitory" or "vaso-dilator" nerves. That it was not stimulation of the vaso-dilator nerves, which, by increasing the amount and the pressure of the blood flowing through the capillaries, occasioned the secretion of saliva, was shown by several experiments, but especially by an observation of Keuchel. This observer found that the alkaloid of the deadly nightshade, viz. atropia, when introduced into the system, exerts such an action, that on stimulating the chorda tympani no secretion of saliva follows; whilst, on the other hand, dilatation of the arteries is produced exactly as under normal circumstances. Other drugs have since been discovered which exert a similar action to that of atropia in paralysing secretory nerves, whilst some are now known which antagonise the action of atropia, and restore the suspended activity of the secretory nerves. From these studies has unquestionably resulted a knowledge of the conclusion, that although the process of secretion is favourably influenced by the vascular dilatation which follows the state of activity of the vaso-dilator nerves, the actual process of secretion is not due to them, but, so far as it is controlled by the nervous system, is directly under the influence of certain nerves which may be termed secretory.

Discoveries which show that Secretion, though influenced by, is not necessarily dependent upon, Stimulation of Nerves going to a Gland.

A knowledge of the facts which I have brought before you hitherto would of itself lead you to suppose that glandular secretion is a process which is in abeyance except under the influence of stimulation of nerves which throw the gland into activity, in the same manner as the quiescent muscle passes into activity normally, only when its motor nerves are stimulated. But this supposition, though it may be in some measure true in the case of certain glands, is not borne out by a study of secreting glands in general—a study which teaches us that whilst the activity of the gland cells may be, and often is, remarkable under the control of the nervous system, it is by no means necessarily dependent upon it. The activity of the gland depends upon the activity of its individual units, the gland cells; and these units may discharge their function so long as they continue to live and are supplied with the nutriment—mineral, organic, and gaseous, which they require.

Leaving aside, at least for the present, any reference to the arguments which may be derived, by analogy, from a study of cell life in general, I would call your attention to the physiological facts which prove the truth of the proposition just enunciated. The first of these facts was discovered by Claude Bernard; to wit, that when all the nerves supplying the salivary glands are divided, there is at first a temporary cessation of secretion, soon followed, however, by an abundant flow of very watery, so-called paralytic saliva.

This result is fully confirmed by similar observations made in the case of other secreting organs, and which establish very fully the greater or less independence of the secreting elements from the control of the nervous system; though unquestionably, in a normal state of the organism of higher animals, the nervous system is continually intervening, both directly by its influence on gland cells, and indirectly by the changes which it produces in the circulation, so as to control the operations of gland cells, and especially to bring them into relation with, and subordinate them to, the work of complex processes of the organism.

What the exact relations of nerve fibres to gland cells may be is yet a matter involved in great doubt. The discovery made by Pflüger of the terminations of nerve fibres in the secreting cells of the salivary glands has not been confirmed by any observers in any vertebrate. Kupffer has, however, unquestionably done so in the case of *Blatta orientalis*, and although as yet objective proof is wanting, we cannot entertain any reasonable doubts that a connection between the ultimate fibrillæ of nerves and secreting cells actually exists. We feel confident that physical, as it were accidental, difficulties have alone hindered the precise determination of the fact.

The Immediate Source of the Nutriment consumed by the Gland Cell.

In the original scheme of a secreting gland, developed first of all by Bowman, then adopted by Goodsir, Carpenter,[1] and many other writers, the essential structural elements taken into account were the following:—1. Epithelial cells lining the secreting cavity of the gland; 2. Sub-epithelial tissue, usually presenting superficially the form of a basement membrane, upon which the cells were placed; and 3. A capillary network in closer relation to the basement membrane, or more superficial part of the sub-epithelial tissue. In harmony with this scheme, the glandular elements were always spoken of as drawing their supply from the blood in the capillaries. The one element which was wanting in that scheme, and which we are able to fit into it, thanks again to the labours of the great physiologist of Leipzig, is the relation of so-called lymph spaces to the other elements. As was first shown by the researches of Ludwig and his school, amongst the modes of origin of the peripheral lymphatics, the most numerous are to be found in connective tissue, and nowhere more abundantly than in the connective tissue of glands, which is everywhere interpenetrated by irregular spaces containing lymph, from whence spring the minutest lymphatics. If we consider, then, the immediate environment of the secreting cell, we find that in close proximity to it is the lymph, which is a transudation from the blood, and upon which the gland cells are directly dependent for all the matters which they require For a certain time, then, the gland cell will be independent of the supply of blood, that is, so long as the lymph surrounding it contains a sufficient quantity of essential matters, of which oxygen is one of the chief, to support its life, or until it becomes so charged with waste products derived from cell life, *e.g.* CO_2, as to interfere with the functions of the latter. It certainly appears that, at least in the majority of cases, it is the secreting cell which modifies, in the first instance, the composition of the lymph which bathes the tissues in proximity to it, rather than the composition of the lymph which modifies the activity of the gland cell. There are some cases nevertheless in which it would appear that the presence of certain constituents in the lymph is the direct cause of the activity or increased activity of the cells.

Secreting Cells present different Appearances, corresponding to different States of Functional Activity. The Researches of Heidenhain.

Amongst the physiologists of Europe who have most enriched science by their researches during the last thirty years is unquestionably Professor Heidenhain of Breslau, who has exhibited his mastery of the physical side of physiology by his classical research on the relations between the heat evolved in and the work done by muscle, and as a biologist able to use in the best manner all the resources of modern histology in the elucidation of bodily function, by the researches to which I wish to direct your attention for a few moments.

The glands imbedded in, or the ducts of which open upon the surface of the mucous membrane of, the alimentary canal, for the most part, are characterised by periods of more or less complete cessation of activity, as judged by the diminution, or absolute cessation, of the secretion which they prepare. This is true of the salivary glands and of the liver, but particularly true of the gastric glands and the pancreas.

Certain of these glands, *i.e.* the salivary glands in some animals, and the stomach and pancreas in all in which they exist, have the task of preparing juices which contain certain so-called unformed or unorganised ferments or *enzymes*, upon which the properties of the secretions in great measure depend. Heidenhain in a long series of investigations, which have been taken part in by certain other scientific men, as by Ebstein and Grützner, by Kühne and Lea, and particularly by Mr. Langley of Trinity College, Cambridge, has shown that the secreting cells of a particular gland, as for instance of the submaxillary gland, of the gastric glands, and of the pancreas, exhibit differences in size, differences in the form and appearance of the nucleus, and differences in the cell contents, corresponding to varied states of functional activity.

Time will not permit my mentioning in detail the results of these observations from which, however, certain general conclusions appear derivable. Thus, a gland cell at rest is usually larger than a similar cell which has been engaged in the process of secretion; from its behaviour to reagents, it usually appears to contain within itself an abundant store of the body or bodies which are chiefly characteristic of the secretion, or closely related antecedents of these, and the amount of undifferentiated protoplasm surrounding the nucleus appears to be at a minimum. On the other hand, the gland cells, which have been secreting for a greater or less period, often, though not invariably, present a diminution in their size, a diminution in the amount of the characteristic bodies previously referred to, and an increase in the protoplasmic constituents of the cell. All facts, histological as well as physiological, seem to point to the following conclusion: that during rest, the cell forms, at the expense of, or as the product of the differentiation of, the cell protoplasm, the bodies characteristic of the secretion; that whilst secretion is going on these leave the gland cell; and that, at the same time, the protoplasmic constituents of the latter increase at the expense of the lymph, to be converted secondarily, either at a later period in that particular act of secretion, or in the succeeding period of inactivity, into specific constituents. The researches of Heidenhain have been conducted upon the glands after these had undergone processes of hardening and straining, the appearances observed indicating changes which, though not identical with, at least corresponded to various conditions of the gland. Kühne and Lea and Langley have, however, studied glands in a living condition, and though the appearances were not identical with those observed by Heidenhain, they entirely confirm these.

I have not time to do more than refer to the fact that in some at least, though probably in all of the cells of glands which produce secretions containing ferments, there are formed at first bodies to which the generic term of "*zymogens*" may be applied, i.e. *ferment generators*, from which a ferment is afterwards set free.

In connection with this part of my subject I may refer to the view, which was at one time held by some, that in secreting glands the gland cell having produced the matter of the secretion was thrown off, discharging its contents into the secretion. This process, when it does occur, must be looked upon as exceptional, and as it were accidental.

Amongst the most striking examples of the success with which physiological experiment and subsequent histological research have been pursued in combination so as to throw light upon the functions of particular cells, I may refer first to the observations of Heidenhain, secondly to those of Nussbaum on the excretion of colouring matters, artificially introduced into the blood, by the secreting epithelium cells of the renal tubules. I have previously referred to the theory of Bowman, according to which the watery and saline constituents of the renal secretion were supposed to be separated by the so-called "*glomeruli*," whilst the organic solids of the secretion were supposed to be separated by the epithelium lining the convoluted tubes.

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[1] Carpenter in his admirable article on "Secretion" in "Todd's Cyclopædia of Anatomy and Physiology."

To this theory was opposed that of Ludwig, according to which the whole of the constituents, watery, saline, and organic, were supposed to be poured out of the vessels of the glomerulus, the amount of water however being far in excess of that contained in the liquids when it reaches the pelvis of the kidney. Ludwig supposed that as the secretion passed over the surface of the epithelium lining the complex tubules, processes of diffusion occurred between it, on the one hand, and the lymph bathing the tissues lying outside of the basement membrane of the tubules on the other, the direction of the current of water being from without inwards. The anatomical evidence adduced by Bowman was of itself well-nigh sufficient to prove the accuracy of his views, which have however been placed beyond all dispute by the following observations: Heindenhain introduced into the blood a solution of sulphindigotate of sodium, usually some time after having divided the spinal cord in the cervical region. On killing the animal some time afterwards and subjecting the kiney to careful examination, it was found that the colouring matter had been accumulated by the epithelium of the convoluted tubules from the lymph bathing the tissues, and which contained so little colouring matter as to appear colourless. If a sufficient time had elapsed after the injection, the colouring matter was found in the form of granules or minute crystals lying on the inner side of the cell in the lumen of the tubules.

Bowman, as I have already mentioned, had in the case of the boa constrictor studied in detail the blood supply to the organ, which, as Jacobson had shown, differs in fishes, birds, and reptiles, from the mode of arrangement prevailing in mammals. Bowman had shown that in the boa the glomeruli derived their blood exclusively from the renal artery, and the convoluted tube exclusively from the common iliac vein. Nussbaum gave absolute completeness to the proof of Bowman's theory by the following remarkable experiment. Experimenting on the newt, in which the blood-supply of the kidney is similar to that of the boa, he found that, when he tied the renal artery, he arrested almost entirely the secretion of water in the kidney, but that the excretion of urea and other solid matters, and amongst others of the colouring matter already used by Heidenhain, viz., indigo carmine, continued. Ligature of the renal branches of the common iliac vein stopped the secretion of organic solids without impeding that of water.

THE MOST RECENT THEORIES ADVANCED IN EXPLANATION OF THE PHENOMENA OF GLANDULAR SECRETION.

Having brought before you the most salient facts with which we are acquainted, which appear to throw the most light upon the general physiology of glandular secretion, I wish, before concluding, to speak of the theoretical views which have been advanced in explanation of a large number of the facts.

In the first place, I have to confess that our ignorance is absolute as to the cause of the specific endowment of different secreting cells, in virtue of which they produce new bodies at the expense of certain of the materials supplied to them by the lymph, or separate particular constituents from the lymph, to the exclusion of others which are equally abundant in the liquid. We express the full measure of our ignorance when we state that the difference in function of different gland cells is due to differences in endowment of the protoplasm of the cell, which in no case is explained by any objective characters of the cell.

The phenomena of the secretion of water, which forms so large a part of every secretion, have given rise, however, to numerous speculations, concerning which I may make a few remarks.

The primitive view that the glands are organs in which is strained off from the blood water holding certain substances in solution has, in a modified manner, found favour with some even to our own days, and appears indeed, at first sight, to be borne out by certain facts. Thus within wide limits the amount of water secreted by the kidney depends upon the pressure of blood in the glomeruli. Any circumstances which will lead to an increase of pressure in these vessels (as increase of blood pressure generally, division of renal nerves, division of the splanchnics, especially when combined with stimulation of the spinal cord), by dilating the branches of the renal artery, will lead to this result. At first this would seem to show that the process of separation of water, in the kidney at least, is but a process of filtration, though a remembrance of the famous experiment of Ludwig, referred to at an earlier period, on the relation between the pressure of secretion of saliva and that of the blood in the arteries, would impose caution in drawing the conclusion. What are the facts, then, relating to the blood pressure in vessels in other organs of the body, and the transudation of liquid from them?

If an increased arterial pressure led *ipso facto* to an increased transudation through the capillary walls, it would follow that the amount of lymph and the pressure of the lymph-stream would rise with the rise of the arterial pressure, but direct experiments on this matter have led to an opposite conclusion. The experiments of Paschutin and Emminghaus, carried out under Ludwig's direction, have shown that when the arterial pressure in the extremities is increased, there is no corresponding increase in the lymph produced. Again, when the chorda tympani is stimulated in an animal into whose blood atropia has been introduced, the vascular dilatation which is produced, and which is produced, and which is then unaccompanied by secretion, does not lead to an increased production of lymph, which would make itself evident by the gland becoming œdematous. How then are we to account for the flow of water through a gland? By ascribing it to an influence which is exerted by the gland cell, in the first place, upon the liquid which environs it, viz. the lymph. And accordingly, even in the case of the glomeruli of the kidney, we conclude that the water is separated as a direct result of the activity of the layer of transparent epithelium cells which cover them. Hering has advanced a strictly physical theory, which would account for the mode in which certain cells exert this influence, by supposing that there is produced within them bodies which, like mucin, have a great affinity for water and which then pass into the secretion; and which therefore lead to a current of water passing through the cell; but the theory is one which cannot be admitted, because, as Heindenhain points out, the passage of water through a gland occurs in cases where there is no constituent in the cells, at all resembling mucin in its affinities or behaviour towards water.

I feel inclined to say that the speculations, necessarily indefinite though they are, of Professor Heindenhain afford the best explanation of the phenomena. Heindenhain starts from the fundamental fact that during secretion only as much water passes out of the blood-vessels of the gland as appears in its secretion, seeing that, however long the process of secretion may continue, the gland never becomes œdematous, nor does the current of lymph from it increase.

The volume of liquid filtered through the blood capillaries adjusts itself exactly to the volume of liquid separated by the cells. This equality in the amount of liquid secreted and filtered appears only explicable on the supposition that the act of secretion is the cause of the current of water—in other words, that the water which the cells lose in the formation of the secretion generates changed in them which can only be compensated for by an abstraction of water from the immediate environment.

Within certain limits, Heindenhain continues, we may form purely physical conceptions of the process. We may conceive, for instance, the whole protoplasm of the cell to have a certain affinity for water. The cells at their contact with the basement membrane may be supposed to be able to abstract water from it; the loss which the membrane sustains will be made up by the lymph, and this again will influence the blood in the capillaries.

The passage of water into the cells will go on until a period of equilibrium is attained; but at that time the current of water from the capillaries through the lymph to the cells will cease. We may conceive further, reasons Heidenhain, that the passage of water out of the cell is hindered by such obstacles to the process of filtration as are represented by resistance opposed to it by the superficial border layer of protoplasm. If we now conceive that, for example as a result of nerve stimulation, the gland cells pour out water, the condition of equilibrium which existed between cell, basement membrane, lymph, and capillary will be disturbed, and a current of liquid set in from the last to the first, and continue as long as the activity of the cells continues.

It is not difficult moreover, Heidenhain remarks, to form physical conceptions of the processes whereby water may be separated from the cell itself. It is conceivable, for instance, that the protoplasm of the cell may contract after the manner which occurs in many infusoria, and which in them leads to the accumulation of water in droplets, forming vacuoles, except that in the case of the secreting cells the water is poured out on the outside and not on the inside of the cells. Or, again, it is possible that on the gland cell passing into the condition of activity an increased production of CO_2 may occur, leading to an increased diffusion of water outwards.

So far, I have quoted Professor Hedenhain, for the most part in his own words. Let me add, however, that the two hypotheses which he advances as possible explanations of the mechanism of secretion of water by the cell rest upon the most probable grounds, as upon the presence of the intra-cellular protoplasmic network which has been so beautifully demonstrated by recent researches, and especially by those of Professor Klein; or, again, upon the fact, proved by the analyses of Professor Pflüger of the gases of the saliva, that there is during secretion great production of CO_2, as shown by the amount of this gas in the saliva being much greater than in the blood, and upon the fact of the remarkable diffusibility of acid solutions.

Reasoning upon a large number of facts, which I have not time to refer to, Professor Heidenhain has come to the conclusion that, quite apart from the nerves which control the vascular supply to a gland, there exist two distinct sets of nerve-fibres in relation to the glandular elements. The first of these, which he terms "secretory," when stimulated, lead to the secretion of water and saline constituents; the second, which he terms "trophic," influence the transformations of the protoplasm of the cell, and thus affect the organic constituents of the secretion.

I do not wish to pronounce a definite opinion concerning this hypothesis, but would remark that the nomenclature proposed by Heidenhain appears to me to be an unfortunate one, especially because it attaches a new meaning to a word which had previously been used by physiologists in a different sense. I refer to the adjective *trophic*, which has always implied "governing nutrition." It appears to me almost inconceivable that if there exist two sets of secretory nerves, the action of each should not profoundly affect the nutrition of the cell protoplasm, though, of course, it is conceivable that they should do so in very different manners.

GENERAL CONCLUSIONS.

The complicated studies, of which I have attempted to give you a brief sketch, have led to our forming certain clear general conceptions in reference to the process of secretion. They have brought into greater prominence the dignity, if I may use the expression, of the individual cell. The process of secretion appears as the result of the combined work of a large number of these units. Each, after the manner of an independent organism, uses oxygen, forms CO_2, evolves heat, and derives its nutriment from the medium in which it lives, and performs chemical operations of which the results only are imperfectly known to us, and which depend upon peculiar endowments of the cell protoplasm, of which the causes are hidden from us. So long as the protoplasm is living, the gland cell retains its power of discharging its functions, and in many cases does so, so long as the intercellular liquid furnishes it with the materials required. In some cases, however, the gland cells are specially sensitive to a variation in the composition of the nutrient liquid, certain constituents of which appear to stimulate the protoplasm to increased activity. In the higher animals the cells, particularly in certain glands, are in relation to nerves which, when stimulated, affect in a remarkable manner the transformations of their protoplasm, leading to an increased consumption of oxygen, an increased production of carbonic acid, an increased evolution of heat, and an increased production of those matters which the cell eliminates and which constitute its secretion.

This historical survey of the growth of our knowledge of the process of secretion exhibits the characteristic features of biological advancement. Comparative anatomy has been the foundation of observation of facts and physical experiment, the road to physiological research. At various stages the value of hypotheses has been well illustrated, and, whenever they have had to make way for the broader and truer interpretations suggested by the accumulation of facts and greater precision of observation, it has been demonstrated that the process of observation is not one of simple sight but of complex ratiocination.

NOTES

A MEDAL and Prize, of the annual value of twenty guineas, has been founded by Dr. Siemens, F.R.S., "with the object of stimulating the students of King's College, London, to a high standard of proficiency in metallurgical science." It is open to those who have, as Matriculated Students, studied in the Applied Science Department for two years, and who, either in their third year, or, if they remain in the Department for three years, in the succeeding year, make metallurgy a special study. The first award will be made at the end of June, 1883, and will depend partly on an essay on some particular subject, partly on a written examination on the metallurgical lectures, and partly on actual work done in the Laboratory. The subject for the essay for 1883 will be the "Manufacture of Steel suitable for Ship and Boiler Plates." The essays are to be illustrated by freehand sketches and mechanical drawings to scale, and must be sent in to Prof. Huntington on or before June 30.

SIR WOODBINE PARISH, K.C.H. and F.R.S., died, towards the close of last week, at Quarry House, St. Leonards-on-Sea, in the 86th year of his age. Sir Woodbine was long engaged in the diplomatic service, though his name is also known in the scientific world. As far back as 1824 he had been elected a Fellow of the Royal Society, and was a member of several learned societies both at home and abroad; he had been a vice-president both of the Geological and Geographical Societies. His name is well known in the scientific world as having brought to this country the remains of the megatherium, the glyptodon, and other fossil monsters from the plains and valleys of South America, and his work on the natural history, &c., of Buenos Ayres and Rio de la Plata received a high encomium from no less an authority than Baron Humboldt.

THE death is announced of Count Lutke, well known in connection with Russian Arctic exploration, especially in the Novaya Zemlya region.

THE next Congress of Electricians will meet in Paris on October 11. The Members will have to deliberate, as we have already stated: (1) on the determination of the length of the mercury column equivalent to the practical ohm; (2) on the construction of lightning conductors, and influence of telegraphic or or telephonic wires on thunder-storms; (3) on the means of establishing a general system of observations for atmospheric electricity; (4) on the opportunity of using the telegraph system for establishing constant communication between a certain number of meteorological observatories. At the same time a Diplomatic Congress will meet on the protection of cables. It is surmised, moreover, that the former will be presided over by M. Cochery, Minister of Postal Telegraphy, and the latter by M. Duclerc, Minister of Foreign Affairs.

WE learn from the *North China Herald* that Sir Robert Hart, the Inspector-General of the Chinese Maritime Customs, has fully granted his assistance to the project of a China coast meteorological service. Formerly a certain Minister of the Customs Officers voluntarily made observations and sent them to M. Dechevrens, the head of the Siccawei Observatory at Shanghai; but these were frequently interrupted by the observers being transferred to other ports. Sir Robert has now directed that the observations at all the ports and lighthouses be sent to Shanghai regularly. A storm warning service is also being organised in Japan under the superintendence of Mr. Knipping.

THE *equatorial coudé* (bent equatorial) invented and designed by M. Lœwy, is in full operation at the Observatory of Paris. Observations are conducted with it, although the clock is not yet in place. The peculiarity is that in consequence of the bending and the use of two reflecting mirrors, the astronomer can observe all the celestial bodies without moving from his table. The reflected rays are sent to the eyepiece through the axial part of the refractor by a fixed mirror. The object-glass is placed at the end of the movable part, which revolves round the axial part